PROCEEDINGS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume 2 JULY 15, 1916 Number 7

ON THE MOBILITIES OF GAS IONS IN HIGH ELECTRIC FIELDS

By Leonard B. Loeb

RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

Received by the Academy, May 20, 1916

Up to the present two theories have been advanced to explain the low order of magnitude of the mobility of the ordinary gaseous ion. The first theory known as the 'cluster' theory is due to Langevin.[1] It assumes that the ion consists of a cluster of neutral molecules surrounding an electron or a positive atomion. The second theory was proposed by Wellisch[2] and is known as the 'small ion' theory. This assumes that both positive and negative ions consist of single charged molecules. In its path through the gas such an ion might be retarded abnormally due to the fact that its charge, acting on neutral gas molecules, drags in a greater number of collisions than would an uncharged molecule.

On the 'cluster' theory it would be expected that if the ion acquired sufficient kinetic energy, it would begin to break up. Such energy might be acquired in traversing a mean free path under a sufficiently high field. The break up would then be indicated by an abnormally high mobility. One would certainly expect such a breaking up of the cluster before field strengths great enough to cause ionisation by collision were reached. The other theory would expect no such break up. It is, however, conceivable on the 'small ion' theory that when the kinetic energy reaches a very high value, near that at which ionisation by collision begins, the negative ion might lose its electron. Experiments which were performed by a number of observers on the gas ions at low pressures (i.e., where with a small value of the field the ion can pick up considerable energy on a long mean free path), indicated

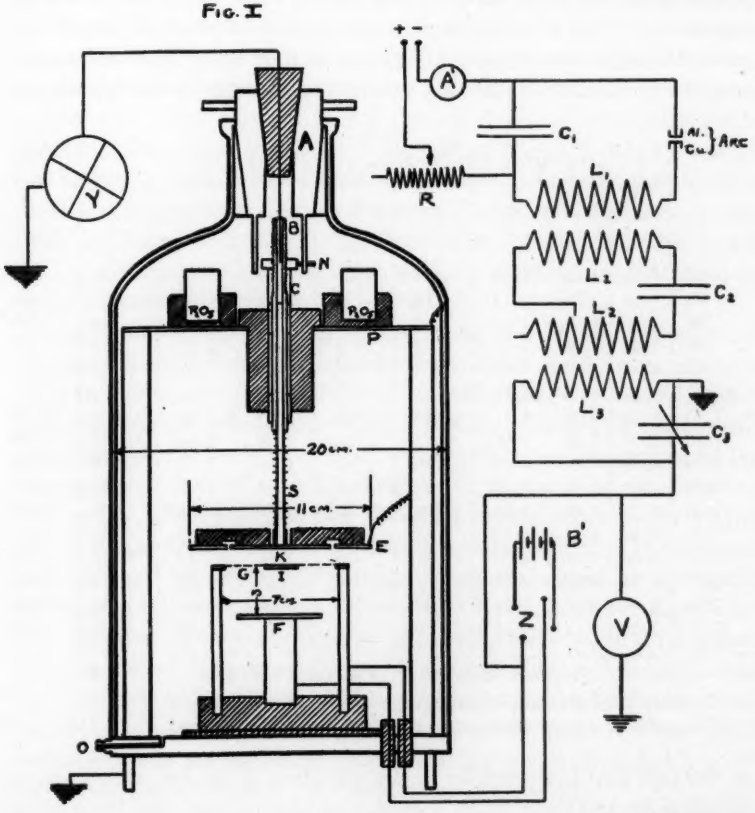
that for the negative ions such a break up did occur. It was consequently thought of importance to see if such a breaking up, or abnormal increase of mobility, of either ion could take place in high electric fields at ordinary pressures. It was considered particularly important to test this point for the case of the positive ion. If in this case an abnormally high mobility were found, only one conclusion could be drawn as to its significance, namely, that the positive ion consists of a cluster. In the case of an abnormally high mobility of the negative ion, the presence of the free electron might make the result ambiguous. It was with this end in view that the work was undertaken at the suggestion of Prof. R. A. Millikan.

Measurements were made by means of the Rutherford[8] alternating current method. With this method the problem resolves itself into one of obtaining high field strengths, e.g., 5000 volts per cm., with alternating fields of frequencies of about 5000 cycles per second. Such frequencies are demanded by the high fields in order to make the distance between the plate and the gauze (i.e., the distance that the ions traverse in one-half cycle), small enough to keep the field between the plate and gauze uniform. These high frequencies were obtained from the resonance circuit of a Chaffee[9] arc. This arc gives nearly undamped sine waves, and is comparatively easy to operate. Using a tertiary circuit as seen in the diagram (fig. 1), an alternating potential difference of 4400 volts at a frequency of 7670 cycles per second was obtained from the arc. The frequency was determined by photographing the spark in a revolving mirror. The mobility-measuring apparatus is similar, as the diagram shows, to that used by other observers. The ions were generated by ionium on a disc I attached to the bottom of the gauze. The whole apparatus was placed inside a silvered bell jar which could be exhausted. Since neither the potential difference nor the frequency could be conveniently varied, the measurements were taken by varying the distance between the plate and the gauze. To do this, the apparatus was so arranged that the upper or electrometer plate could be raised or lowered from the outside of the jar. The distances between the plate and the gauze were plotted as abscissae, and the electrometer deflections as ordinates. The point where this curve intersected the axis of abscissae was taken as the critical distance which the ions could just traverse in the time of one-half period of oscillation. From the value of this distance d, the mobility U of the ion is obtained at once with the aid of the equation $U = \pi N d^2/E$, in which N is the frequency and E the maximum value of the potential.

The measurements were made in air, which was carefully dried

with phosphorous pentoxide in the later measurements. The variations of the values of the mobilities as shown in the table were in a large measure attributable to insufficient drying in the earlier determinations. Fluctuations in the operation of the arc, together with slight uncertainties as to the value of the constants of the alternating potential difference, might possibly have caused an error of ten per

FIG. I



cent in the determination of the absolute value of the mobilities. The actual error, however, is probably much less than this. By means of check determinations, using the 120 volt 60 cycle city alternating potential, which gave strictly normal values of the mobilities, the possibility of any instrumental errors was precluded.

The results obtained are shown in Table 1. In the first column are the frequencies of alternation in cycles per second. In the next

two columns the critical distances in centimeters are given. The next two give the mobilities. The next column gives the pressure in millimeters of mercury. The next columns give the values of the mobilities reduced to 760 mm. pressure. The last two columns give the field strengths for the maximum values of the fields in volts per centimeter.

The measurements were carried up to fields of 12,450 volts per centimeter in air at atmospheric pressure, and to somewhat lower field strengths at pressures as low as 300 mm. of mercury. The results could not be extended to higher field strengths nor lower pressures, because

TABLE I

IONIC MOBILITIES OBTAINED WITH CHAFFEE ARC

N	d_+	d−	u_+	u−	p	U_+	U−	X_+	X−
					mm.				
60	0.99	1.38	1.10	2.12	760	1.10	2.12	121	87
60	1.00	1.26	1.13	1.78	756	1.12	1.76	120	96
*60	1.00	1.28	1.13	1.85	748	1.12	1.84	118	93
60	1.33	1.56	1.95	2.70	578	1.48	2.05	91	77
7670	0.50		0.97		750	0.98		12,450	
7670	0.55	0.64	1.17	1.59	746	1.19	1.62	11.460	9,750
7670	0.54	0.61	1.07	1.37	747	1.09	1.40	11,550	10,200
7670	0.50	0.65	1.25	1.80	735	1.28	1.86	12,450	9,600
7670	0.66	0.84	1.70	2.74	534	1.27	1.95	9,580	7,430
*7670	0.78	1.02	2.36	4.04	436	1.36	2.32	8,000	6,000
*7670	0.85	1.01	2.72	3.88	430	1.54	2.18	7,346	6,160
7670	0.82	1.10	2.58	4.29	416	1.41	2.29	7,610	5,670
7670	0.85	0.90	2.67	3.07	384	1.35	1.53	7,340	6,910
7670	0.80	0.86	2.51	2.90	382	1.26	1.46	7,800	7,260
†7670	1.11	1.22	4.78	5.80	304	1.96	2.32	5,610	5,160
Mean Mobilities at 760 mm.						1.33	1.89		

* These readings were taken with very dry air.

† The value of the + mobility only estimated here, not determined.

of the fact that irregularities in the operation of the arc caused serious sparking beyond these limits. It can be clearly seen that for a range of field strengths extending from 90 volts per centimeter to 12,450 per centimeter in air at atmospheric pressure, *there is neither for the positive nor the negative ion any marked increase in the absolute value of the mobility. It is also to be noticed that the mobility of the negative ion shows no abnormal increase relative to that of the positive ion.* In other words, in fields of nearly half the value of the sparking field strength in air (30,000 volts per centimeter at 760 mm. pressure), and in fields where occasional sparking did actually occur (due to irregularities of

the arc), one *finds that the mobilities are absolutely normal.* Even at pressures as low as 300 mm., where the potential fall per mean free path is larger still, there is no abnormal mobility.

These results as regards the mobility of the positive ion for high values of the product, field strength times mean free path, seem to be in direct contradiction to the results of Todd,[4] who, working at very low pressures and small field strengths, got an apparent abnormal increase in the mobility of the positive ion. The value of this product may be expressed as the ratio X/p, to which it is proportional, where X is field strength in volts per centimeter and p is the pressure in millimeters of mercury. The value of X/p for which Todd[4] observed the abnormality of the positive ions is 2.6. The values of X/p at which I worked are as high as 18.0. The latter's results agree, however, with those of Wellisch[5] recently obtained at pressures as low as 0.05 mm. of mercury. Wellisch[5] found no abnormal mobilities of the positive ions for values of X/p as high as 34.5. This value of X/p is close to that for ionisation by collision at this pressure.

The results obtained by me with negative ions for high pressure for values of X/p near where ionisation by collision must begin, seem to be contrary to the results of most of the observers who worked at low pressures with much smaller values of X/p. Townsend,[6] basing his assertions on the work of Lattey,[7] states that for low pressures the abnormal mobility, i.e., the breaking up of the cluster, should begin at $X/p = 0.1$. This appears to be in direct contradiction to the results of the writer obtained with air at pressures as low as 300 mm., and with $X/p = 16.0$, where strictly normal negative mobilities are found. In view of this contradiction, I worked over the results of Kovarick,[8] who obtained abnormally high negative mobilities at low pressures, to see whether the appearance of the abnormally high mobilities in his work was a function of X/p. The initiation of these abnormal mobilities was found to be a function of p rather than of X/p, which indicated, since my pressures were much higher than Kovarick's,[8] that the two sets of results were not necessarily in conflict.

All of the above facts are in accord with the recent results of Wellisch[5] on the mobilities of negative ions at low pressures. He found that, even at the lowest pressures at which he worked, the negative carriers were in part at least perfectly normal negative ions. This was the case for values of X/p close to the value of X/p for ionisation by collision at those pressures. He also found increasingly great numbers of free electrons in air as the pressure was reduced below 8 cm., but no intermediate negative ions. These free electrons could not, according to Wellisch,

be detected by the other observers because of their low frequencies of alternation. The result was that the curves of these observers gave them apparent abnormal increases in the mobility of the negative ion, which increased in value with decreasing pressures. In my experiments these electrons were of course absent, and so no apparent abnormal increase was obtained.

The conclusion to be drawn from these results seems to be that the 'cluster' theory, which has until now been most generally accepted, is not correct. This forces us to accept the 'small ion' theory in some form or other.

Summary.—1. The mobilities of positive ions have been determined in electric fields very nearly strong enough to cause ionisation by collision at atmospheric pressures and have been found to be perfectly normal within the limits of error of the measurement.

2. The mobilities of the negative ions have also been determined, under the same circumstances, with the result that they not only showed no relative abnormal increase in value over those of the positive ion, but also showed a perfectly normal absolute value of the mobility.

3. These results, though at variance with those of most observers at low pressures for the negative ions, are in good agreement with recent results of Wellisch,[5] and likewise lead to the conclusion that the 'cluster' theory is no longer tenable.

[1] Langevin, Thesis, *Ann. Chim. Phys., Paris*, 1902.
[2] Wellisch, *London, Phil. Trans. R. Soc.*, A, 209 (1909).
[3] Rutherford, *Cambridge, Proc. Phil. Soc.*, 1898.
[4] Todd, *Phil. Mag., London*, June, 1913.
[5] Wellisch, *Amer. J. Sci., New Haven*, May, 1915.
[6] Townsend, *Electricity in Gases*, 1914.
[7] Lattey, *London, Proc. R. Soc.*, A, 84, 1910.
[8] Kovarick, *Physic. Rev., Ithaca*, 1911.
[9] Chaffee, *Boston, Proc. Amer. Acad.*, 47, No. 9, 1911.

THE RELATION OF MYELIN TO THE LOSS OF WATER IN THE MAMMALIAN NERVOUS SYSTEM WITH ADVANCING AGE

By Henry H. Donaldson

WISTER INSTITUTE OF ANATOMY AND BIOLOGY, PHILADELPHIA

Read before the Academy, April 17, 1916. Received, June 6, 1916

Starting from birth, the water-content of the mammalian body diminishes with age, and the same statement holds for the several anatomical systems which compose the body (Lowrey[1]). My own studies have been made on the albino rat in which the changes in the water-

content of the central nervous system have been followed as a phenomenon of growth (Donaldson[2, 3, 4, 5]).

The progressive loss in the percentage of water in the brain and in the spinal cord is closely linked with age, although it can be slightly modified by nutritive conditions. The age of the animal becomes therefore the most important datum when a determination of the percentage of water is to be made.

In Chart 1 the change, on age, of the percentage of water in the brain and in the spinal cord of the male albino rat is shown by graphs. The observed mean values for groups of cases are indicated by small dots, for the brain, and by small triangles, for the cord. The respective graphs were determined by formulas devised by Dr. S. Hatai, and based on the observed group values (Donaldson[6]). Along the graph for the brain are seen four large dots. These represent observations on the human brain—taken from Koch and Mann, and from Weisbach.[7, 8]

Table 1 gives the percentage of water and the age of the human brains here used and also the equivalent rat ages.

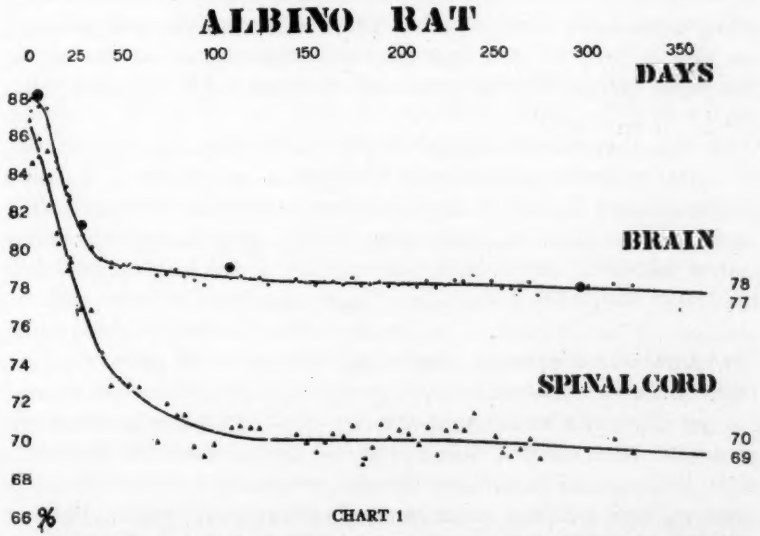


CHART 1

Showing the percentage of water, on age, in the central nervous system of the albino rat. The upper graph gives the values for the water in the brain as determined by the formulas (Hatai—in 'The Rat,' Donaldson,[6]—1915). The lower graph gives the corresponding values for the spinal cord, determined in the same way.

The small black dots indicate the observed values for the several age groups for the brain—and form the data for the formulas. The small black triangles have a like value in relation to the spinal cord.

The large black dots along the graph for the brain represent the human data as given in Table 1.

TABLE 1

Percentages of water in the human brain

Koch and Mann, (K); Weisbach, (W)

HUMAN AGE, YEARS	OBSERVER	PERCENTAGE OF WATER	EQUIVALENT RAT AGE DAYS
Birth	(W)	88.3	Birth
2.0	(K)	81.1	26
9.5	(W)	79.2	115
25.0	(W)	(77.0)	290
	(K)	(77.8)	

We find that the span of life for man is about thirty times as long as that for the rat and hence by reducing any observed human age to one-thirtieth of its value we obtain the corresponding rat-age to which it is equivalent. These equivalent ages are given in Table 1. As is seen from the chart, when the water determinations for man are entered in accordance with this age relation they agree well with those for the rat. We conclude, therefore, that essentially the same process is occurring in the brains of both man and the rat—only in the rat it is proceeding thirty times faster than in man. These relations have been pointed out here in order to show that the conclusions reached at the end of this paper will probably apply to man, although directly based on the rat.

The fact that there is a progressive loss of water in the brain and in the spinal cord with advancing age having been established, it is desirable to consider the organs themselves and to find how this loss is distributed within them. The following analysis applies both to the brain and to the spinal cord—but, for convenience, it will be given as if for the brain alone. The brain is very largely composed of nerve cells or neurons. When reduced to its simplest terms a neuron consists of a cell body—with a principal outgrowth—the axon. All axons are free from visible myelin sheaths when young—but as they mature many, but not all, acquire rather thick sheaths of myelin. Myelin is a morphological term used to designate these sheaths, which are composed mainly of lipoids and which give the nerve fibers a white color. Broadly speaking, there is little or no visible myelin present in the brain at birth—while with advancing age it increases rapidly, and at maturity forms a considerable portion of the entire brain.

It is my purpose to determine by this study whether the loss of water, which occurs in the brain between birth and maturity, takes place equally in the cell body and its axon, on the one hand, and in the myelin sheaths, on the other, or whether this loss is unequally distributed.

By way of introduction the following data from the literature are presented (de Regibus[10]).

In man at birth the brain has about 88% of water and both the gray substance (in which cell bodies are abundant), and masses of fibers alone (later to become 'white substance'), have the same percentage. At maturity, however, the case is very different, as will be seen from Table 2.

TABLE 2

Percentage of water. Human brain

	CORTEX (GRAY)	CALLOSUM (WHITE)
	per cent	*per cent*
At Birth	88.0	88.0
At Maturity	86.0	70.4

According to this table the (gray) cortex has lost 2 points and the (white) callosum, 17.6 points in the process of maturing.

It is never possible to obtain at maturity the cortex, or any other gray mass, without an admixture of some myelinated fibers, and I have therefore credited one point of the loss, noted in the water content of the cortex, to the presence of such fibers.

According to this assumption the mature gray substance. when the myelin of the myelinated fibers is excluded, contains 87% of water.

In the computations which follow, the neurons (=cell bodies and axons), without myelin, are assumed to have 87% of water.

The fact that the fibers without myelin (see Table 2) have at birth a high percentage of water (88%) while at maturity, after myelination, they have lost 17.6 points, indicates that either axons of this type have a peculiar capacity for losing water, or that the accumulation of myelin has caused the reduction observed.

To obtain a notion of the approximate distribution of the water between the myelin and neurons proper, it is necessary to have data on the relative abundance of these two constituents of the brain.

In 1913 W. Koch and M. L. Koch[11] made a study of the chemical composition of the brain of the albino rat at six ages, between birth and maturity, and of the spinal cord, at one age. The data thus obtained are those which will be utilized here. The authors determined seven fractions:—proteins, organic extractives and inorganic constituents, which three taken together, we shall designate, protein (or non-lipoid); and phosphatides, cerebrosides, sulphatides and cholesterol, which four taken together, we shall designate, lipoid.

These data give us at each age, therefore, the protein and the lipoid

present in the brain, or to be a little more exact, we should say the lipoid and the non-lipoid fractions. The lipoid (in part) represents the myelin sheaths, while the protein, with part of the lipoid, represents the cell bodies and their unsheathed axons.

With the exception of the one day group, the ages for which analyses were made are given in Table 3.

At one day—or practically birth—it is found that the lipoid is present to the extent of 0.31 or nearly one-third of the weight of the protein. There is, however, no visible myelin at this age, so it is concluded that this proportion of the total lipoid is normally associated with the protein and is not to be included in the lipoid which forms the myelin sheaths. We have treated the data for the later age groups in accordance with this relation—and in each case have taken from the total lipoid found an amount equal to 0.31 of the protein found. The remaining amount of lipoid is assumed to be that used for the sheaths.

In Table 3, the column (2) headed 'Corrected Protein' gives the observed protein (non-lipoid) plus 0.31 of itself—and the column (3) headed 'Corrected Lipoid' gives the observed lipoid less the amount of lipoid added to the protein.

TABLE 3

To show for five brains of the Albino rat and for one spinal cord the percentage of water in the myelin as computed according to the method described. The protein and lipoid are given in percentages of the total dry substance (Based on Table 2. Koch and Koch)[11]

(1)	(2)	(3)	(4)	(5)	(6)	(7)
				PERCENTAGE OF WATER		
AGE IN DAYS	CORRECTED PROTEIN. PER CENT	CORRECTED LIPOID. PER CENT	PROPORTION OF LIPOID FOR DIFFERENT AGES. LIPOID AT 20 DAYS = 1	Entire brain observed.	In neurons = Protein (C) (assumed).	In myelin = lipoids (C) (computed).
			Brain			
10	93.80	6.2		86.5	88	63.8*
20	88.88	11.12	1.	82.5	87	46.5†
40	82.86	17.14	1.5	79.4	87	42.7
120	75.11	24.84	2.2	78.4	87	52.4
210	76.36	23.63	2.1	78.1	87	47.4
Average of 20 to 210 days.....						47.8
			Spinal Cord			
120	52.92	47.08	4.2	70.4	87	51.0

* First traces of myelin. † Myelin well shown.

In Table 3 the data are given in five age groups for the brain and in one age group for the spinal cord. It is to be noted that the 10 day brain group—which stands just at the beginning of the myelin formation—is here excluded from the discussion and we begin the comparisons which are to be made, with the 20 day brain group.

In the brain series (with one exception) the corrected protein diminishes and the corrected lipoid increases with advancing age. Between 20 and 210 days the proportion of the lipoid doubles. We have, in column (5), the observed percentage of water in the brain as a whole. It is assumed, as previously noted, that the corrected protein (neurons, in the strict sense: = both cell-bodies and axons) have 87% of water. From these several data we can compute the percentage of water to be assigned to the corrected lipoid, which represents the myelin.

The method of computation may be illustrated by the data for the 20 day group. Reference to Table 3 shows that, at this age, there is 1 part of lipoid (11.12%) to 8 parts of protein (88.88%). This gives 9 parts, representing the entire brain and having 82.5% of water. The product, $9 \times 82.5 = 742.5$. We assume that the 8 parts of protein have 87% of water. The product, $8 \times 87 = 696$. The 1 part of lipoid, representing the myelin, will then have a percentage of water equal to the difference of these products: $742.5 - 696 = 46.5\%$.

The values thus obtained are entered in column (7), and, taken together, the four entries give a mean value of 47.8% of water for the myelin.

In this connection it should be noted that the spinal cord, which has about twice as much lipoid as the brain at the same age, gives also a similarly low value for the water in the myelin—51%.

The significance of these results lies not in the particular percentage of water here determined for the myelin—as that depends on the percentage of water assumed for the protein—but in the similarity of the values found in all the five cases examined.

We conclude from these results that there is no evidence that the cell bodies and their unsheathed axons suffer any significant loss of water between birth and maturity, and that the progressive diminution in the water content of the entire brain and spinal cord is mainly due to the accumulation of myelin—with a water content of about 50%. Moreover, the myelin must be regarded as a more or less extraneous substance, having but little significance for the characteristic activities of the neurons. As the diminution in the percentage of water in the central nervous system is preëminently a function of age, and as it appears to be due almost entirely to the formation of the myelin, it

follows that the myelin formation is also a function of age. A glance at the graphs in Chart 1, and at column (4) in Table 3, will show that the most active production of myelin, as indicated by the rapid loss in the percentage of water, occurs early, i.e., during the first twentieth of the life span in both the rat and man.

[1] Lowrey, L. G., *Anat. Rec.*, **7**, 143–168 (1913).
[2] Donaldson, H. H., *J. Comp. Neur. Psychol.*, **20**, 119–144 (1910).
[3] Donaldson, H. H., *Ibid.*, **21**, 129–137 (1911).
[4] Donaldson, H. H., *Ibid.*, **21**, 139–145 (1911).
[5] Donaldson, H. H., *Ibid.*, **21**, 161–176 (1911).
[6] Donaldson, H. H., The Rat, *Mem. Wistar Inst. Anat. Biol., Philadelphia*, No. 6 (1915).
[7] Koch, W. and Mann, S. A., *Arch. Neur. Psychiat.* (Mott), **4**, 201–204 (1909).
[8] Weisbach, A., *Med. Jahrbücher* **16**, Nos. 4 & 5, 1–76 (1868).
[9] Donaldson, H. H., The Rat, *Mem. Wistar Inst, Philadelphia*, No. 6 (1915).
[10] De Regibus, C., *Torino, Atti. R. Acc. Med.* **6**, 323–328 (1884).
[11] Koch, W. and Koch, M. L., *J. Biol. Chem.*, **15**, No. 3, 423–448 (1913).

DIFFERENTIAL MITOSES IN THE GERM-CELL CYCLE OF DINEUTES NIGRIOR

By R. W. Hegner and C. P. Russell

ZOÖLOGICAL LABORATORY, UNIVERSITY OF MICHIGAN

Received by the Academy, May 26, 1916

One of the most interesting and important periods in the germ-cell cycle of certain insects is that during which oögonia give rise to nurse cells and oöcytes. While such a period does not occur in certain insects, such as the paedogenetic fly, *Miastor*, in which the nurse cells are of mesodermal origin (Kahle, 1908; Hegner, 1914), perhaps in the majority of the members of this class, the growth of the egg is preceded by the formation of nurse cells from which the oöcyte derives most of its contents.

Many investigators have studied the origin and history of the cellular elements within the ovaries of insects, but in only one family, the Dytiscidae, have clearly defined visible differences been discovered between the nurse cells and the oöcytes at the time of their origin from the same mother cells. In the diving beetle, *Dytiscus marginalis*, Giardina (1901) discovered true differential mitoses which result in the derivation of one oöcyte and fifteen nurse cells from each ultimate oögonium. In this case there is a series of four mitoses during each of which one cell divides unequally; the larger daughter cell is characterized by the presence of an extra-nuclear 'chromatic ring' and leads to the formation of the oöcyte; the other gives rise only to nurse cells. Giardina supposes that this peculiar chromatic ring consists of part of the chromatin

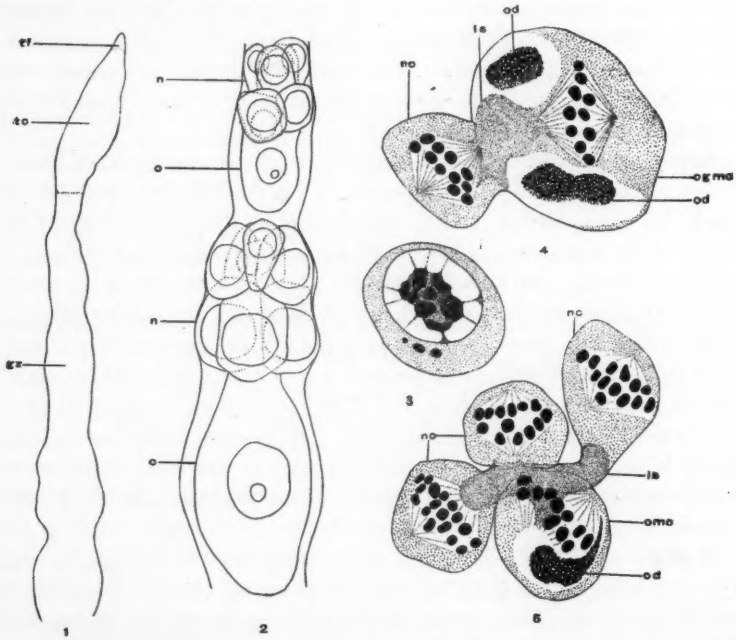
contained in the nucleus of the ultimate oögonium. He found that a chromatic network remains in the nucleus after the chromosomes are formed and that the ring results from the condensation of this network. Günthert (1909) repeated Giardina's work and confirmed it in almost every respect. Regarding the origin of the chromatic ring, he believes that it may consist of chromatin that has separated from the chromosomes. Debaiseaux (1909) also studied these differential mitoses in *Dytiscus marginalis* but added very little to Giardina's account. His most interesting conclusion is that the material of the chromatic ring may really be nucleolar in nature.

Several writers had noted peculiarities in the ovarian cells of beetles before Giardina published the results of his investigation but did not work out their history. Thus Will (1886) described and figured the origin of the nurse cells in the beetle, *Colymbetes fuscus*, but his work is lacking in detail, and Korschelt (1886) actually observed the chromatic ring is *Dytiscus marginalis* but failed to determine its real significance. Since Giardina's investigations were published many attempts have been made to find differential mitosis of a similar sort in other insects, but thus far without any success. Kern (1912) found a group of granules in the oögonia of the ground beetle, *Carabus nemoralis*, and a similar group in the oöcyte of *C. glabratus* but none of the intervening stages were seen and the connection between these granules and differential mitoses is therefore doubtful.

It seems particularly fortunate, considering these many failures, that a species belonging to another family of insects has at last been found in which visible substances can be observed during the differential mitoses. The material on which the following report is based consists of over thirty ovaries from the whirl-i-gig beetle, *Dineutes nigrior*.[1] These were obtained by the senior author[2] at Woods Hole, Massachusetts, on August 28, 1915. Some of them were fixed in Meves' modification of Fleming's solution, some in Bouin's picro-formol solution, and some in Carnoy's acetic-alcohol-chloroform-sublimate mixture. Longitudinal sections were cut 5 microns thick and stained with Heidenhain's iron-haemotoxylin. The general relations of the nurse cells and oöcytes were obtained from *in toto* preparations.

One of the ovarioles from an ovary of *Dineutes nigrior* is shown in longitudinal section in figure 1. It consists of three general zones, the terminal filament (*tf*), the terminal chamber (*tc*) and the growth zone (*gz*). The anterior end of each ovariole is attached to the dorsal body wall by a long terminal filament made up of epithelial cells. The oögonia multiply and differentiate into nurse cells and oöcytes within

the terminal chamber. After differentiation has occurred, the oöcytes and their accompanying nurse cells lie at the anterior end of the zone of growth. Each oöcyte is nourished by seven nurse cells which lie in a nurse chamber anterior to it. The arrangement of these nurse cells is shown in figure 2, *n*.



1. Outline of an ovariole. *tf* = terminal filament, *tc* = terminal chamber containing undifferentiated oögonia; *gz* = zone of growth. (× 140)

2. A portion of growth zone, drawn from in toto preparation, showing eight cell stages. *n* = 7 nurse cells arranged in nurse chamber; *o* = oöcyte connected with nurse chamber. (× 225)

3. A single ultimate oögonium from anterior end of terminal chamber. (× 2000)

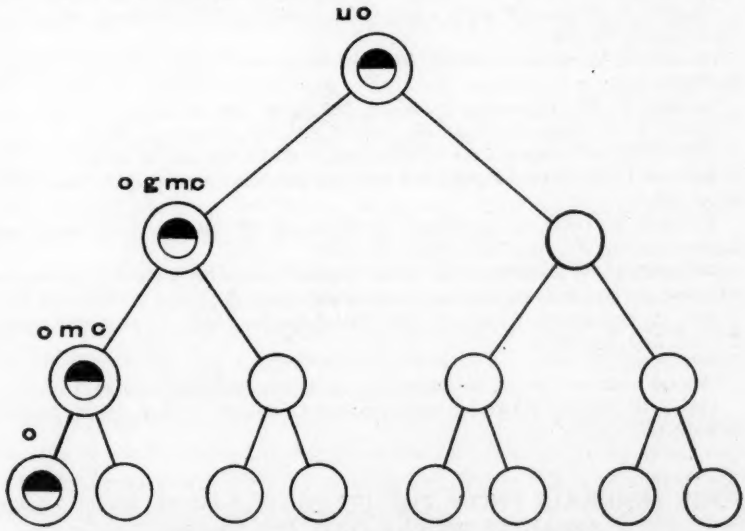
4. Two-cell stage. Metaphase of second division. *nc* = nurse cell; *ogmc* = oöcyte grandmother cell containing oöcyte determinant *od*; *is* = intercellular strand. (× 2000)

5. Four-cell stage. Metaphase of third division. *nc* = nurse cells, *omc* = oöcyte mother cell, containing oöcyte determinant *od*; *is* = intercellular strand. (× 2000)

The large terminal chamber contains at its distal end many oögonia ready to undergo differentiation. These ultimate oögonia (fig. 3) are characterized by a nucleus containing a deeply staining central mass of chromatin from which many fine strands radiate to the nuclear membrane. A few small darkly staining bodies are sometimes present in the cytoplasm. The two daughter cells of such an ultimate oögonium are shown

in figure 4 preparing for division. One of these cells (*nc*) is smaller than the other and gives rise to nurse cells only. The other, the oöcyte grandmother cell (*ogmc*) is larger and contains within its cytoplasm portions of a deeply staining body that we do not hesitate to homologize with the 'anello cromatico' of Giardina and which we may call the 'oöcyte determinant.' Between the two cells is an intercellular strand (*is*) which represents the remains of the spindle fibres of the previous mitotic division.

A four-cell stage such as is illustrated in figure 5 proves that the division of the oöcyte grandmother cell (fig. 4, *ogmc*) is differential since one cell, the oöcyte mother cell (fig. 5, *omc*) has received all of the



6. Diagrammatic representation of oöcyte differentiation in Dineutes nigrior. *uo* = ultimate oögonium, containing oöcyte determinant within its nucleus; *ogmc* = oöcyte grandmother cell having similar nuclear characteristics; *omc* = oöcyte mother cell, *o* = oöcyte. Plain circles indicate nurse cells.

oöcyte determinant (*od*), whereas the other three (*nc*) are smaller and lack this body, and are destined to become nurse cells. The intercellular strands are quite conspicuous here and leave no doubt in the mind of the observer as to the origin of the four cells from a single cell, the ultimate oögonium. Each of these four cells undergoes a single mitotic division resulting in the formation of seven nurse cells and one oöcyte, the latter containing the oöcyte determinant.

The most conspicuous difference that we have discovered between the

origin of the oöcyte in *Dineutes nigrior* and in *Dytiscus* is in the number of differential mitoses; in *Dineutes nigrior* there are only three (fig. 6) whereas in *Dytiscus* there are four.

As a result the oöcyte of *Dineutes nigrior* is accompanied by only seven nurse cells, that of *Dytiscus* by fifteen. The origin and history of the oöcyte determinant are of considerable interest and importance because of their bearing on the subject of the physical basis of heredity. A more detailed study of our material is now being made and the ovaries of *Dineutes nigrior* and of several other species promise to furnish a means of clearing up the details of differential mitoses in these insects.

Debaisieux, P., Les débuts de l'ovogenese dans le Dytiscus marginalis, *La Cellule*, **25** (1909).

Giardina, A., Origine dell' oocite e delle cellule nutrici nel Dytiscus, *Intern. Monatschr. Anat., Leipzig*, **18** (1901).

Govaerts, P., Recherches sur la structure de l'ovaire des insects, *Arch. biol., Paris-Bruxelles*, **28** (1913).

Günthert, T., Die Eibildung der Dytisciden, *Zool. Jahrb., Jena*, **30** (1910).

Hegner, R. W., Studies on Germ Cells, I and II, *J. Morph., Boston*, **25** (1914).

Kahle, W., Die Paedogenese der Cedidomyiden, *Zoologica, Stuttgart*, **21** (1908).

Kern, P., Ueber die Fortpflanzung und Eibildung bei einigen Caraben, *Zool. Anz., Leipzig*, **40** (1912).

Korschelt, E., Ueber die Entstehung und Bedeutung der verschiedenen Elemente des Insectenovariums, *Zs. wiss. Zool., Leipzig*, **43** (1886).

Maziarski, S., Sur la persistance de résidus fusoriaux pendant les nombreuses générations cellularies du cours de l'ovogenesese de Vespa vulgaris, *Arch. Zellforsch.*, **10** (1913).

Will, L., Oogenetische Studien, I. Die Entstehung des Eies von Colymbetes fuscus, *Zs. wiss. Zool.*, **43** (1886).

[1] We are indebted to Mr. A. W. Andrews for the identification of this species.

[2] Mr. R. W. Hegner is indebted to the Bache Fund for assistance in preparing the material on which this report is based.

SOME MINERALS FROM THE FLUORITE-BARITE VEIN NEAR WAGON WHEEL GAP, COLORADO

By Esper S. Larsen and Roger C. Wells

U. S. GEOLOGICAL SURVEY, WASHINGTON, D. C.

Received by the Academy, May 27, 1916

In the summer of 1912 Messrs. W. H. Emmons and E. S. Larsen[1] made a hasty examination of the fluorite-barite vein which passes through the hot springs near Wagon Wheel Gap, Colorado. Since that time this vein has been extensively prospected and a considerable amount of fluorite has been produced. In the summer of 1915, in the course of the reconnaissance geologic study of this region under the direction of Dr. Whitman Cross, Mr. Larsen made another brief visit to the deposit and collected two specimens of material not before recognized in the deposit.

One of these specimens proved to be the unusual mineral gearksutite, and the other was made up of a peculiar kaolinite and a new fluoride-sulphate, *creedite*. It is the purpose of the present paper[2] to describe these minerals.

The vein is entirely in Tertiary volcanic rocks made up of lava flows and tuff beds of rhyolite and quartz latite, with subordinate andesite. It can be followed for perhaps half a mile and is variable in width but is not uncommonly wide enough to make the mining of the fluorite profitable. Much of the vein filling is nearly pure fluorite but locally barite is abundant. The wall rock shows considerable alteration and commonly carries much disseminated pyrite.

Gearksutite.—The lower tunnel of the mine, whose portal is about 100 feet east of one of the hot springs, follows the vein and some distance from the portal it passes through a large body of soft, highly altered rock, giving rise to considerable caving. This soft material is a highly altered rhyolite or quartz latite, probably a tuff, which carries very abundant balls, up to several inches across, of snow white, powdery gearksutite. These balls are easily separated from the altered rock and resemble a very fragile chalk or kaolinite. When wet they become plastic and resemble ordinary dough and are without grit.

A microscopic examination shows that the material is homogeneous and is made up of an aggregate of shreds so minute as to be recognized only with a high magnification. It has a mean index of refraction of 1.454 ± 0.003 and a moderate birefringence. The specific gravity measured by the pycnometer method is 2.768.

This agrees with the properties of gearksutite from Greenland,[3] which "occurs in white, chalky aggregates of minute fibers or prisms which are optically negative and have a moderate axial angle. X is normal to the fibers and Y makes a large angle with them.

The mineral is probably monoclinic with X = b.

$\alpha = 1.448 \pm 0.003$, $\beta = 1.454 \pm 0.003$, $\gamma = 1.456 \pm 0.003$."

A chemical analysis of the material from Wagon Wheel Gap shows its identity with gearksutite. In the following table Analysis 1, by R. C. Wells, is of gearksutite from Wagon Wheel Gap, Colorado. Analysis 2, by G. Lindström, is of gearksutite from Ivigtut, Greenland, and Analysis 3, by Hillebrand, is of that mineral from St. Peters Dome, Colorado. Column 4 gives the theoretical composition,[4] on the basis of the formula $CaF_2.Al(F,OH)_3.H_2O$. with F:OH = 2:1.

Analyses of gearksutite

	1*	2	3	4
Al	15.11	15.37	15.20	15.1
Fe	tr.	0.30		
Ca	22.41	21.02	22.30	22.4
Mg	tr.	0.16		
Na	0.04	1.06	0.10	
K	0.07	0.23	0.04	
Cl		0.20		
F	41.00	41.81	42.07	42.9
O	5.09	4.82	4.83	4.5
H_2O-	0.44			
H_2O+	15.20	15.03	15.46	15.1
	99.36	100.00	100.00	100.0

* SiO_2 none, Al_2O_3 28.49, Fe_2O_3 tr., CaO 31.37, MgO tr., Na_2O .05, K_2O .08, F 41.00, H_2O- .44, H_2O+ 15.20, less O for F 17.27, total 99.36.

Although the direct determination of fluorine, in which the fluorine was weighed as CaF_2, yielded 41.0 per cent in Analysis 1, no great accuracy is claimed for this determination. The agreement of the other constituents in the three analyses, however, shows the practical identity of the minerals, the chief difference being that a small amount of Ca is replaced by Na in the Greenland mineral. All four analyses agree very well with the formula $CaF_2Al(F,OH)_3.H_2O$. with F : OH = 2 : 1.

Heretofore gearksutite has been described only as an alteration product of cryolite, but at Wagon Wheel Gap no cryolite has been recognized and the gearksutite is believed to have been formed by a metasomatic alteration of the rhyolitic wall rock much as kaolinite or sericite is formed. Indeed, on the first visit to the deposits the gearksutite was mistaken for kaolinite. It seems probable that the gearksutite was developed in the wall rock by the ascending hot solutions which deposited the fluorite in the vein.

Creedite, a New Mineral.—A specimen collected from the dump of one of the upper workings of the fluorite mine proved to be made up in large part of a new mineral. The name 'Creedite' is proposed for this mineral from its occurrence near the center of the Creede quadrangle of the United States Geological Survey.

Creedite makes up about two-thirds of the specimen and is in white to colorless grains and poorly developed crystals up to 5 millimeters across, imbedded in a dull white kaolinite, with a very little barite. The kaolinite is believed to be derived from the alteration of some unknown mineral.

Creedite is nearly colorless, has a hardness of about $3\frac{1}{2}$, intumesces before the blow pipe, showing the calcium flame, and finally fuses to a

white enamel. It is somewhat slowly but completely soluble in acid. Its specific gravity as measured on the powder by the pycnometer method is 2.730.

Most of the grains show no good crystal development, but a few are stout prismatic crystals with an acute rhombic cross section. One crystal showed curved and striated pyramidal faces. The faces are dull, curved, and imperfect, and not suitable for accurate measurements. The angle between the prism faces measured under the microscope on the cross section of a prism gave 68° and is believed to be accurate to within about 1°. However, a measurement on a second prism with distinctly curved faces gave only 59°. The mineral has a perfect cleavage parallel to the elongation of the prisms and bisecting the obtuse angle of the prismatic faces.

Creedite has distinctive optical properties and was first recognized by them. Extinction angles can not be measured accurately on account of the imperfect development of the crystals. However, cleavage fragments show sensibly parallel extinction and the emergence of an optic axis very nearly normal to the cleavage plane. Prismatic sections normal to the cleavage (bisecting the acute angle of the faces) show extinction angles of about 41° ($Z \wedge$ elongation), the emergence of **Y**, sensibly normal, and irregular twinning. Crystals lying on the crystal face show extinction angles ($Z \wedge c$) of $42\frac{1}{2}° \pm 1°$. The mineral is therefore believed to be monoclinic with the faces (110), perfect cleavage (100), $Y = b$, and $Z \wedge c = 41°$; (100) is a twinning plane.

The mineral is optically negative. The axial angle as measured on the axial angle apparatus is:

$2V_{li} = 64° 30' \pm 10'$, $2V_{na} = 64° 22' \pm 10'$, $2V_{Tl}$, $64° 20' \pm 10'$.

Other less satisfactory sections gave

$2V_{na} = 64° 18' \pm 20'$ and $2V_{na} = 64° 50' \pm 30'$.

The dispersion was perceptible only on one optic axis.

The indices of refraction were measured by the immersion method by matching in a liquid and immediately measuring the index of refraction of the liquid at the same temperature; the probable error should be less than ± 0.001. All of the grains give the same value showing that the mineral is homogeneous.

$$\alpha = 1.461, \quad \beta = 1.478, \quad \gamma = 1.485.$$

The axial angle computed from the indices of refraction is 65° which agrees very well with the measured values.

Material for a chemical analysis was carefully purified by heavy solution (bromoform) and a careful microscopic examination showed only a very little kaolinite present. The analyses by R. C. Wells gave the following results:

Analysis and molecular ratios of Creedite

					2.
Al	11.58	427	210	2 × 105	11.0
Ca	23.98	599	295	3 × 98	24.4
SO_4	18.32	191	94	1 × 94	19.5
O*	3.97	248	122	1 × 122	3.2
H_2O-	0.72				
H_2O	11.08	615	303	3 × 101	11.0
F*	30.35	1591	784	8 × 98	30.9
	100.00				100.00

* Calculated on the basis of a summation of 100%.

This leads to the formula $CaSO_4.2CaF_2.2Al(F,OH)_3.2H_2O$ with F: OH 2: 1 and the mineral has the composition of gearksutite with one molecule of $CaSO_4$ added. The composition corresponding to this formula is given in column 2. Creedite is one of the very few minerals described that contain both sulphate and fluoride as essential constituents.

Isotropic Kaolinite-Like Mineral.—The creedite is imbedded in a small amount of a kaolinite-like mineral with rather unusual properties, which is believed to have been derived from some unknown mineral. In the hand specimen it is a white powder, in the thin sections it is clear, in large part sensibly isotropic, and can be distinguished from the Canada balsam in which it is imbedded, only by testing its index of refraction. Locally it shows birefringence in wavy streaks, probably due to strain in the soft mineral or to incipient crystallization. It has an index of refraction of 1.557 ± 0.003. When immersed in a liquid with that index of refraction it is at first clouded but the oil rapidly penetrates its pores, the mineral becomes clear, and the grains can be found only by the colored borders formed by the Schroeder van der Kolk test for the index of refraction. The specific gravity of the powder measured by the pycnometer method is 2.548.

A partial analysis by Roger C. Wells gave:

SiO_2	Al_2O_3	CaO	MgO	F	Ign.	Total
44.2	40.2	0.3	tr.	none	15.5	100.2

Imbedded in this mineral and like it derived from some mineral that has disappeared, are spherulites of an undetermined mineral with rather strong birefringence, positive elongation, and a mean index of refraction of about 1.50.

[1] Emmons and Larsen, The hot springs and the mineral deposits of Wagon Wheel Gap, Colorado, *Economic Geology*, 8, 235 (1913).

[2] Published with permission of the Director of the United States Geological Survey.

[3] Unpublished manuscript by Esper S. Larsen.

[4] Dana, *A System of Mineralogy*, sixth edition, p. 181, 1892.

THE PROCESSES TAKING PLACE IN THE BODY BY WHICH THE NUMBER OF ERYTHROCYTES PER UNIT VOLUME OF BLOOD IS INCREASED IN ACUTE EXPERIMENTAL POLYCYTHAEMIA

By Paul D. Lamson

PHARMACOLOGICAL LABORATORY, JOHNS HOPKINS UNIVERSITY

Received by the Academy, June 9, 1916

In a previous communication to the PROCEEDINGS[1] I reviewed the work of the past on polycythaemia, and reported a series of experiments carried out in the hope of determining if possible the process by which the number of erythrocytes per unit volume of blood is increased in acute experimental polycythaemia. Since then further experiments have been undertaken, some of which have already been reported.[2] The results of these experiments, and others about to be published, are collected here in a brief summary of this work up to the present time.

This complicated problem may be divided into four main parts:

(1) The causes capable of producing an increase in number of erythrocytes per unit volume of blood.

(2) Localization of these processes.

(3) The manner in which the number of erythrocytes is increased.

(4) The mechanism by which the red corpuscle content of the blood is controlled.

As previously pointed out, the causes of polycythaemia are very numerous, and have received a great deal of attention, but little work has been done concerning the localization and manner in which this increase in number of erythrocytes is brought about.

From experiments previously reported, the present author concluded that the liver is the organ which is responsible for the changes in number of erythrocytes produced by the intravenous injection of epinephrin.[3] This conclusion has been confirmed by the following experiments here summarized.

(1) Epinephrin when injected into a dog or cat intravenously in doses of 0.9 mg. per kilo causes an increase in number of erythrocytes of 1.5 to 2.0 millions per cubic millimeter of blood in from five to ten minutes, lasting about one half hour and then gradually returning to normal.

(2) Removal of the stomach, intestine, mesentery, omentum, pancreas, and spleen, either singly or all together has no appreciable effect on this reaction to epinephrin.

(3) A head-thorax animal does not respond to epinephrin.

(4) The addition to the head-thorax animal of the liver, supplied by arterial blood only, gives an animal which responds to epinephrin.

(5) Removal of the liver from the circulation by ligation of the hepatic artery and shunting the portal bood around the liver by means of an Eck fistula, gives an animal in all respects normal except for the absence of the liver. Epinephrin in such an animal causes no increase in number of erythrocytes per unit volume of blood.

(6) In an animal in which all arterial blood supply to the liver is shut off by ligation of the hepatic artery (the portal circulation being left intact), epinephrin causes no increase in number of erythrocytes. But if one half hour or later, after the injection of epinephrin, arterial blood is again allowed to reach the liver by removal of this ligature, the number of erythrocytes is immediately increased as if a fresh dose of epinephrin had been injected into the animal.

(7) In an animal in which the arterial blood supply to the liver is shut off by ligation of the hepatic artery the injection of epinephrin into the portal vein causes an immediate increase in number of erythrocytes.

From these experiments it is evident that it is necessary for the liver to be present in the circulation for polycythaemia to take place. If epinephrin does not reach the liver as is the case when the hepatic artery is ligated (the epinephrin being probably adsorbed to a great extent in its passage through the other portal organs) no polycythaemia takes place. But if in this condition, epinephrin is allowed to reach the liver by injecting it into the portal vein, polycythaemia takes place at once.

As regards the manner in which the number of erythrocytes per unit volume of blood is increased following the injection of epinephrin we know that it must take place in one or more of the following ways.

(1) A decrease in plasma volume.

(2) An absolute increase in number of erythrocytes by (*a*) a formation of new erythrocytes, (*b*) a division of erythrocytes (a theoretical consideration), (*c*) bringing erythrocytes into the circulating blood from a reservoir or reservoirs in the body.

The following facts have been experimentally demonstrated.

(1) The number of erythrocytes per unit volume of blood is increased.
(2) An absolute increase in number of erythrocytes takes place.
(3) Plasma volume is decreased.
(4) Total blood volume is decreased.
(5) The average volume of the individual erythrocyte is decreased.
(6) The haemoglobin content of the average erythrocyte is decreased.
(7) No evidence of newly formed erythrocytes has been obtained.
(8) No evidence of the division of erythrocytes has been obtained.
(9) Proof that erythrocytes temporarily stored somewhere in the body, are brought into the circulation, has been obtained by deduction from the above facts, and a condition in which erythrocytes are stored has been experimentally demonstrated as follows.

If the hepatic artery is ligated and epinephrin intravenously injected, the number of erythrocytes per unit volume of blood remains unchanged, but at the same time the plasma volume is markedly decreased.[4] This is theoretically impossible without a destruction or a storing of erythrocytes somewhere in the body. No evidence of their destruction has been found. There is no haemolysis, and no haemoglobinuria. Furthermore polycythaemia may be repeatedly produced in the same animal.

The localization of this reservoir of erythrocytes was experimentally carried out in the following manner. As shown above, the liver is responsible for the increase in number of erythrocytes which follows the injection of epinephrin, and a certain portion of this increase is due to cells brought forth from some reservoir. In the experiments in which the hepatic artery is ligated and no increase in number of erythrocytes follows the injection of epinephrin, but where a storing of erythrocytes must take place, opening the hepatic artery is immediately followed by an increase in number of erythrocytes in the generally circulating blood. Or if in this same condition where the hepatic artery is ligated, epinephrin is injected into the portal vein, causing a constriction of the liver capillaries, and a decrease in liver volume, an immediate increase in number of erythrocytes occurs.

It has thus been experimentally shown, that the polycythaemia following the intravenous injection of epinephrin is due to some action of the liver. A condition has been brought about by ligation of the arterial blood supply to this organ and the intravenous injection of epinephrin, in which erythrocytes must be stored away. By returning the arterial blood supply to the liver, or by causing a constriction of the liver capillaries by the injection of epinephrin into the portal vein, both of which procedures offers mechanical conditions favorable

for forcing out erythrocytes lying dormant in the capillaries of this organ, the red blood corpuscles known to have left the general circulation immediately appear in the blood stream causing an increase in number of erythrocytes per unit volume of blood. From these experiments it is concluded that the liver acts as a reservoir for erythrocytes.

The process by which the liver increases the number of erythrocytes is thought to be a loss of plasma from the liver capillaries, together with a constriction of these vessels driving the erythrocytes on into the blood stream. It has been experimentally shown that the liver capillaries constrict with epinephrin, causing a sudden diminution in size of the liver. This does not occur except to a slight extent when the hepatic artery is tied, and epinephrin injected intravenously, in which condition no increase in number of erythrocytes takes place. It is also of interest to note, as more fully described in a previous paper,[5] that Mautner and Pick[6] have shown that the livers of cats and dogs constrict very markedly when epinephrin is perfused through them or intravenously injected into these animals. Rabbits however do not respond in this way to epinephrin, the liver capillaries not contracting. They also show no increase in number of erythrocytes per unit volume of blood when epinephrin is intravenously injected. Thus we have two types of animals, one the cat, whose liver responds to epinephrin by constriction of its capillaries and gives a polycythaemia, the other the rabbit, whose liver does not respond to epinephrin and in which this substance produces no polycythaemia.

Concerning the mechanism which normally controls the red corpuscle content of the blood, my view was previously expressed that the adrenal glands play an important part in this process. Certain experiments were carried out in which the polycythaemia occurring in the normal animal did not occur in animals after the removal of the adrenals. Two additional experiments have been made which have strengthened the belief in this view.

As previously shown, certain nervous phenomena are capable of producing a polycythaemia. This increase in number of erythrocytes must occur either by some entirely different process from that caused by the injection of epinephrin, or by some nervous influence directly or indirectly affecting the liver. Experiments in which the nerves going to the liver were stimulated, showed no increase in number of erythrocytes. Furthermore in the experiments above reported, in which epinephrin is shown to have no effect on the number of erythrocytes per unit volume of blood in the rabbit, nervous phenomena known to cause polycythaemia in cats (which respond to epinephrin) caused no

increase in number of erythrocytes in these animals. If polycythaemia is due to some direct nervous influence it might be expected that in these animals in which the nervous condition is intact, nervous stimuli as fright, would cause an increase in number of red corpuscles.

As the direct stimulation of the nerves to the liver causes no increase in number of erythrocytes, and as in animals in which epinephrin causes no polycythaemia nervous stimuli also cause no increase in number of red corpuscles, it appears probable that the polycythaemia following the stimulation of certain nerves is not due to a direct nervous influence, but to a reflex stimulation of the adrenals and a secondary action of epinephrin on the liver.

[1] Lamson, P. D., These PROCEEDINGS, **1**, 521-525 (1915).

[2] Lamson, P. D., *J. Pharm. Exp. Therap.*, **7**, 169-224 (1915).

[3] The terms red count, number of red corpuscles, etc., will be used occasionally for the sake of brevity, instead of the more exact term, number of erythrocytes per unit volume of blood.

[4] Lamson, P. D., and Keith, N. M., *J. Pharm. Exp. Therap.*, **8**, 247-251 (1916).

[5] Lamson, P. D., *J. Pharm. Exp. Therap.*, **8**, 167-173 (1916).

[6] Mautner, H., and Pick, E. P., *Münchener med. Wochenschr.*, Nr. 34, S. 1141, 1915.

THE INFLUENCE OF MORPHIN UPON THE ELIMINATION OF INTRAVENOUSLY INJECTED DEXTROSE IN DOGS

By I. S. Kleiner and S. J. Meltzer

DEPARTMENT OF PHYSIOLOGY AND PHARMACOLOGY, ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

Read before the Academy, April 17, 1916. Received June 9, 1916

The sugar content of the circulating blood is practically constant. This constancy is upheld by some stable mechanism which controls the supply of sugar to and its elimination from the circulation. Some time ago[1] we communicated the instructive fact that after the intravenous injection of four grams of sugar per kilo of body weight the sugar content of the blood returns to nearly normal again in about 90 minutes. These animals had ether anesthesia during the operation and afterwards a subcutaneous injection of morphin. The question presented itself, whether the anesthetics which were used influenced the rate of elimination of the added sugar through the kidneys and the blood capillaries, or, to express it in the terms of the hypothesis which was uppermost in our minds, whether the anesthetics affected the *physiological permeability* of the endothelia and epithelia which are concerned in the process of elimination.

We tested first the action of morphin alone. Two series of experi-

ments were made. Eight dogs received morphin, about ten milligrams per kilo body weight. Ten dogs were operated under local anesthesia produced by cocain or ethylchloride; no anesthesia was needed during the infusion of the sugar solution. In all cases of both series four grams of dextrose per kilo body weight was given, the duration of the intravenous infusion being in all cases about one hour. The dextrose was given in a 20% solution, the solvent being water, except in four non-morphinized dogs in which the dextrose was dissolved in an M/4 solution of sodium sulphate—for reasons which will be stated later.

The resulting differences were striking and instructive. We shall give first the urinary results. In the morphinized dogs the elimination of the sugar through the urine was considerable. In eight experiments the average quantity of sugar in the urine secreted in two hours and a half (that is from the beginning of the injection to one hour and a half after the end of it) amounted to 63% of the injected sugar, 80% being the largest and 52% the lowest quantity. The average quantity of sugar in the urine of six non-morphinized dogs (those which received the dextrose in water), in two hours and a half, amounted only to about 17% of the injected sugar, about 30% being the highest and about 4% the lowest quantity.

There was, however, a proportionate difference between the two series of dogs also in the volume of the secreted urine. In the morphinized dogs the average of the injected dextrose solution was 137 cc. and of the secreted urine—197 cc.; that is, the quantity of the secreted urine exceeded that of the injected sugar solution, while in the non-morphinized dogs the average of the injected dextrose solution was 187 cc. and of the secreted urine only 83 cc.; that is, the quantity of the secreted urine was much smaller than that of the injected sugar solution. It could then perhaps be assumed that the morphinized dogs eliminated more sugar merely because they have secreted more urine. On this account experiments were made on four non-morphinized dogs in which the dextrose was dissolved in 1/4M solution of sodium sulphate. That produced a greater diuresis; here the relation of the volume of the secreted urine to the volume of the injected dextrose was reversed: 212 cc. of dextrose solution injected and 281 cc. of urine secreted. Nevertheless, the relation of the elimination of sugar in the urine was not reversed. In fact, in those four experiments with non-morphinized dogs the elimination in the urine of sugar was even diminished; it amounted on the average only to 9.2% of the injected sugar, about 13% being the highest and 7% the lowest quantity.

These three series of experiments show, then, that morphin increases

considerably the elimination through the kidney of intravenously injected sugar, and increases moderately the volume of the secreted urine.

In these various series of experiments also the sugar content of the blood was tested and compared with one another. Tests were made before the intravenous injection of sugar and at the end of it, and thirty minutes, sixty minutes and ninety minutes after the end of the injection. The results were in a certain sense opposite to those observed for the urine. The averages for six morphinized dogs were as follows: 0.24% before the injection, 0.99% at the end, 0.57% after thirty, 0.41% after sixty and 0.32% ninety minutes after the end of the injection. The average for six non-morphinized dogs (dextrose in water) were: 0.16% before the injection, 0.6% at the end, 0.19% after thirty, 0.16% after sixty and 0.16% ninety minutes after the end of the injection. These data mean that in morphinized dogs, after intravenous injection of dextrose, the sugar content of the blood returns to the normal level much slower than in non-morphinized dogs.

However, in consideration of the fact that morphin alone causes a moderate hyperglycaemia (with a very slight glycosuria) two experiments were made on non-morphinized dogs which had at the start a hyperglycaemia. These animals received intravenously one gram of dextrose per kilo, which brought the glycaemia to 0.35%; then four grams more per kilo were injected. At the end of this injection the hyperglycaemia was 0.45%; thirty minutes after the end, about 0.16%; after sixty minutes, 0.15%; and after ninety minutes, 0.15%. This shows that without morphin the sugar of the blood returns more rapidly to the normal level even when the intravenous injection of four grams of sugar per kilo is started while the sugar content of the blood is temporarily on a level higher than that due to morphin. The elimination of sugar in the urine amounted to about 18.5% of the entire quantity of the injected sugar.

Our experiments show the following facts. 1. Morphin, in the dose employed in our experiments, increases considerably the elimination through the kidneys of intravenously injected dextrose, although the increase in the volume of secreted urine is comparatively moderate. Without morphin the elimination of sugar is comparatively small even if the volume of the secreted urine is considerably increased by the addition of sodium sulphate to the intravenously injected dextrose. 2. Morphin retards the return of the sugar content of the blood to its previous level. The originally slightly higher level of glycaemia which is produced by morphin itself does not seem to be responsible for either the slow return of the glycaemia or for the greater elimination through the kidney.

If we assume with Heidenhain and others, as most physiologists now do, that urine is produced by a 'vital' activity of the cells of the glomeruli and of the convoluted tubules, we may perhaps express our findings by the statement that morphin increases the 'physiological permeability' of the kidney cells while it decreases the same kind of permeability of the endothelia of the capillaries of other tissues of the body.

[1] Kleiner and Meltzer, *Amer. J. Physiol.*, **33**, 17 (1914); also Kleiner, *J. Exp. Medicine*, **23**, 507 (1916).

THE WORK OF THE AMERICAN METEOR SOCIETY IN 1914 AND 1915

By Charles P. Olivier

LEANDER McCORMICK OBSERVATORY, UNIVERSITY OF VIRGINIA

Received by the Academy, May 29, 1916

The year 1915 saw a very great increase in the interest in the study of meteors, which was evidenced by the large number of observations made by members of the American Meteor Society. This gratifying increase became largely possible on account of a grant to Dr. S. A. Mitchell of the Leander McCormick Observatory from the J. Lawrence Smith fund of the National Academy of Sciences. This appropriation, which was made in April, 1915, permitted the work of the Meteor Society to obtain wider publicity by the publicaion and distribution of bulletins, maps and blanks to prospective members.

As a consequence, it is believed that the largest amount of systematic work ever done in one year in America, was sent in; the results of these observations have been prepared for publication and are now awaiting printing. Briefly, this publication will contain the results from 540 observations made by 4 persons in 1914 and from 5003 observations made by 36 persons in 1915. While most of these 36 persons are amateurs, five have had astronomical training, one is a colonel in the U. S. Army, one is an observer of meteors in the U. S. Weather Bureau of wide experience, three are students in astronomy at the University of Virginia, and several others are trained in various scientific lines which would make their work the more valuable. The observers were stationed in 17 states, 2 provinces of Canada, and one in the Argentine Republic. It might be added that the Meteor Society has members in several foreign countries and several dozen more in America from whom no reports have yet been received, while a week rarely passes without a new person applying for membership.

From the 5543 observations of meteors mentioned, we have been able

to deduce 139 radiants of sufficient accuracy to calculate parabolic orbits for the meteor streams they represent. These orbits are contained in full in one of the tables. Following this is a table containing 81 less certain, but probably existant radiants, for which as yet no orbits are calculated. Other tables contain analyzed data of the distribution of meteors as to magnitudes and average durations of their times of visibility. There is also a table containing a few real heights, which were obtained in August, 1915, between Richmond, Va., University of Virginia, and Washington, D. C. This latter work we hope to repeat, on a larger scale and under better conditions, during the summer of 1916. The text of the publication contains details as to the organization, plans and methods of reduction of the work. It further contains full explanations as to the derivation and use of the figures found in the tables. Actual directions to the members were omitted since these had been printed at great length both in *Popular Astronomy* and also in Bulletins 2–5 of the Meteor Society, which were distributed to all members and applicants.

The present publication and the two similar ones previously prepared by me will bring up the number of results to 440 parabolic orbits of meteor streams, based on about 14,000 meteors. The peculiar value of this contribution lies in the fact that a fairly uniform plan has been followed by all the observers, and that the results were computed and deduced by one person using the methods and care with which other astronomical work is handled in all regular observatories.

It may be of interest to mention the various methods used to reach amateurs who might care to join in the work. At the very organization of this society, the coöperation of the members of the Meteor Section of the Society for Practical Astronomy was secured by the appointment of the writer as director of the latter. Then last spring a number of articles were published by Dr. S. A. Mitchell and myself calling attention to the desirability of coöperation on the part of all amateurs. These articles appeared in the *Scientific American*, *Journal of the Royal Astronomical Society of Canada*, and *Popular Astronomy;* by reprinting them in part or in whole, the press of the country assisted in bringing our work to the notice of amateur observers and, as a result, wide publicity was secured. Not less than 200 people have written letters on the subject to date, over half desiring to join in making observations. It goes without saying that many have never been heard from again, but so many have worked with real enthusiasm and success, that it would seem that the future of meteoric astronomy is brighter at the present time than ever before, so far as America is concerned.

This is peculiarly fortunate at such an epoch when similar societies in Europe must be greatly reduced in membership and activity.

Having secured the approbation and support of the National Academy of Sciences for the coming year, through a further grant from the J. Lawrence Smith fund, it is hoped that the results for 1916 will surpass those for the previous year, and indeed a good start has been made in that direction. We still need and desire the help of other persons interested in such work and a cordial invitation is again extended to them.

THE LIGHT EXCITATION BY SLOW POSITIVE AND NEUTRAL PARTICLES

By A. J. Dempster

RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

Received by the Academy, June 12, 1916

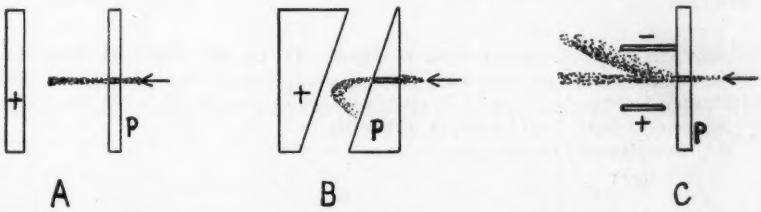
The speed at which an electron begins to excite light, and the character of the light emitted by slow electrons has formed the subject of several recent papers;[1] the corresponding problem for slow positive rays has not been carefully investigated. The only papers on the subject are those by Stark[2] and Wehnelt[3] who detected a luminosity probably due to positive rays with potential differences as low as 50 volts on the tube.

The light excitation by positive and neutral rays of greater energy is included in the many papers on the light emission of canal rays. These particles have energies corresponding to a fall of potential of from 500 volts up, depending on the pressure, the low speeds necessarily being at a relatively high pressure. The Doppler effect in these canal rays in hydrogen shows a dark space between the displaced and undisplaced lines, and this may be explained by assuming that those particles in the rays which have less than a certain speed (corresponding to 50–80 volts fall of potential for the hydrogen atom) are unable to excite light.

The method used in the present experiments was to ionize hydrogen by electrons from a Wehnelt cathode and to allow the positives thus made to pass through a slit in a plate behind the cathode (P in the figures) into a second chamber where their light emission and the deflection of their path by electric fields could be studied undisturbed by the light and the electric fields in the main tube. In one tube (*A*) a plate was placed in the second chamber at right angles to the beam of rays so that by charging it positively the positive rays could be stopped and turned back; in a second arrangement (*B*) the rays were allowed to enter an oblique retarding field so that the paths of the positive particles

would be parabolas; and in a third experiment (*C*) the particles passed between the plates of a condenser which would deflect any charged particles sideways out of the beam.

The phenomena depended greatly on the pressure of the gas in the tube. If the pressure were very low so that the positives made practically no collisions with the molecules of the gas, the rays, which were followed by the luminosity they make in the gas, could be stopped entirely by the directly opposing field or bent into a parabolic path by the inclined field or deflected sideways by the cross electric field. *A* and *B* indicate the appearance with high vacua. The rays could be detected with potentials as low as 30 volts on the tube, although they were then very faint. To stop the rays, or rather to reduce their speed so that they are unable to cause light, opposing potentials were required which were practically the same as those which originally gave the particles their speed. We conclude that *very slow positive rays are still able to excite light, certainly with a speed corresponding to less than 5 volts.*



This fact needs to be considered in connection with the above mentioned experiments on the lower limit at which electrons excite light.

If the pressure of hydrogen is taken a little higher so that some of the positives make collisions, the phenomena are more complicated. If the positives are then deflected out by a cross field, there is an undeflected bundle of light left, due to the neutral rays which have been formed from the positives. *C* indicates the appearance at about 0.005 mm. of mercury pressure when the two bundles are of approximately equal intensity. With the arrangements *A* and *B* there is superimposed on the deflected beam a luminosity continuing up to the opposing electrode. If the pressure is taken higher still, the positive bundle becomes fainter till finally the neutral bundle alone is present. Two things are of special interest. First, *the neutral rays can also excite light at very slow speeds.* The lowest speed at which the neutral bundle has been detected is 50 volts, and it is possible that that is a lower limit at which the neutrals begin to excite light. With potentials of 1500 volts, when the light is much stronger, photographs of the spectra

of the positive and neutral bundles showed the hydrogen series lines in both.

The second point is that there is a sharp separation between the two bundles at the proper pressure. This shows that light is not emitted only during the return of a corpuscle to a positive centre, as very often assumed, for the neutrals cause light in a region where all free corpuscles have been swept out by the field. Also the sharp separation of the bundles shows that at these pressures changes take place slowly, so that if a positive were formed in the neutral bundle it would be deflected out before becoming neutralized again. We are thus led to conclude that *light excitation may occur directly because of the collision of a neutral particle with a neutral molecule of the gas.* We may regard the light emission as taking place during the rearrangement of the electrons in the atom after one has been detached by the collision (Stark's theory), or we may retain the picture underlying Bohr's theory, if we regard the displaced electron as not leaving entirely the centre to which it is attached.

[1] E. Gehrcke and R. Seeliger, *Verh. D. Physik. Ges.*, **15**, 897 (1913); H. Rau, *Ber. Phys.-med. Ges., Würzburg*, Feb. 1914; J. Frank and G. Herz, *Verh. D. Physik. Ges.*, **16**, 512 (1914); J. C. MacLennan and J. P. Henderson, *London, Proc. R. Soc.*, A., **91**, 485 (1915).

[2] J. Stark, *Leipzig, Ann. Physik.*, **13**, 390 (1904).

[3] A. Wehnelt, *Ibid.*, **14**, 464 (1904).

AN APPARENT DEPENDENCE OF THE APEX AND VELOCITY OF SOLAR MOTION, AS DETERMINED FROM RADIAL VELOCITIES, UPON PROPER MOTION

By C. D. Perrine

OBSERVATORIO NACIONAL, ARGENTINO, CÓRDOBA

Received by the Academy, June 3, 1916

The position of the solar apex which had been derived from the stars of class B both from proper motions and radial velocities differed so much from the apex derived from radial velocities of A stars, that an investigation was undertaken of the radial velocities of all of the spectra classes. The discordances were not only between the spectral classes as a whole but appeared to be between the results from northern and southern stars as well.

In the course of the work it was observed that frequently there appeared to be marked discordances between the radial velocities of stars whose proper motions also differed in size and sign. This, and the desire to test the effect of differences of distance as evidenced by

proper motion, led to the separation of the stars in the classes A, F, G, and K according to proper motion in right ascension into four classes—0 to $^s.0049$, $^s.0050$ to $^s.0099$, and $^s.0100$ and over, designated respectively in the table as small, medium, and large, and the stars having 'contrary' parallactic signs, i.e., negative signs in the first and fourth quadrants of right ascension and positive signs in the second and third quadrants.

The sky was divided into areas approximately 30° square and the mean of the stars in each region treated as one observation. The

μ_α	SPECTRAL TYPE	NORTH			SOUTH		
		A	D	$V_\odot$	A	D	$V_\odot$
				km.			*km.*
Large	A	242°	+ 7°	−14.2	306°	+14°	−20.7
	F	254	+ 5	−23.2	238	−11	−13.1
	G	268	− 4	−36.4	287	− 6	−13.9
	K	277	+18	−26.3	298	+19	−30.0
		260	+ 6	−25.0	282	+ 4	−19.4
Medium	A	260	− 4	−22.6	305	+20	−14.5
	F	253	−17	−25.5	307	+19	−19.9
	G	272	+ 7	−37.6	246	− 6	−44.1
	K	262	−34	−25.2	232	+59	−21.3
		262	−12	−27.7	272	+23	−25.0
Small	A	260	+35	−17.7	261	+ 3	−20.2
	F	270	+37	−15.0	264	+32	−20.7
	G	271	+27	−14.6	242	+28	−12.4
	K	265	+32	−19.6	258	+39	−22.4
		266	+33	−16.7	256	+26	−18.9
	B	254	+27	−17.0	288	+31	−24.8
Contrary	A	209	+37	−12.2	240	+18	−18.9
	F	292	+58	−13.3	204	+32	− 8.4
	G	207	+80	−19.0	147	+ 1	− 6.4
	K	288	+44	−14.2	292	+60	−20.3
		249	+55	−14.7	221	+28	−13.5
	Omitting G	263	+46	−13.2	245	+37	−15.9

results of least-squares solutions are given in the table. A and D are the right ascension and declination respectively, of the solar apex and $V_\odot$ the velocity of the solar system. As the stars of class B practically all fall in the small proper motion division, the results from them are included in that division in the table.

The principal conclusion from this investigation is that the position of the solar apex and the solar velocity appear to vary with the proper motions of the stars used in the determination, at least for the fainter naked-eye stars of the northern sky, the declination approaching the

well-known ellipsoidal axis and the solar velocity increasing as we go from the stars of small and 'contrary' proper motion to those of medium and large proper motion.

Such variations of the position of the apex although apparently connected with distance or other conditions, would seem to point ultimately to some form of rotary or spiral motion among the stars themselves.

The details of the investigation, which is based upon Campbell's well-known catalogs of about 1300 radial velocities, will be published in the *Astrophysical Journal*.

CHANNELED GRATING SPECTRA, OBTAINED IN SUCCESSIVE DIFFRACTIONS

By C. Barus

DEPARTMENT OF PHYSICS, BROWN UNIVERSITY

Received by the Academy, June 13, 1916

The great variety of channelled spectra obtained, when white light is successively diffracted by two gratings, are referrable to the fringes of the diffraction of homogeneous light, observed *outside* the principal focal plane, on a spectrometer. In other words, if light of a given pure color (sodium, mercury) is used, a single grating suffices. Each line of the spectrum is resolved into well defined groups of fringes, if it is observed either in front of or behind the principal focal plane. The arrangement of fringes varies in marked degree with the distance of the plane observed, from the latter. If reflecting gratings are used, there is no other possible source of interferences; but reflecting and transmitting gratings show the phenomenon equally well.

After finding how easily the Fresnellian interferences of two virtual slits could be reproduced in the telescope and observed on either side of (before or behind) the plane of the sharp slit images, it seemed reasonable to suppose that the diffraction of a slit could also be produced and exhibited in this way; but the availability of this anticipation is attended with much greater difficulty. The image of a very distant slit does indeed show separated diffraction fringes on either side of the principal focal plane in the observing telescope. But they move right and left with the eye, in the same direction and in this respect do not at once recall the phenomena under consideration. Usually the blurred image, out of focus, is stringy, without definite structure.

To obtain sharp stationary fringes from an image of the slit, this image must be produced by the diffraction of a grating, having a dis-

persing power above a certain minimum. Thus in a grating of less than 7,000 lines to the inch, the undeviated slit image and the image of the first order are not clearly resolved, unless the slit is very fine. In the second and higher orders, however, the resolution is very pronounced and the fringes stationary.

The resolution of fringes is equally manifest in front of, or behind the principal focal plane, so that if a weak convex lens is added to the objective of the telescope, the succession of fringes is found with an outgoing ocular; if a weak concave lens is added to the objective, with an ingoing ocular, starting in each case near the principal focus. As the fringes increase in size they in turn subdivide, as if each fringe were a new slit image, capable of undergoing secondary diffraction. Beyond these secondary fringes no further resolution was detected.

Returning to the work with two successive gratings and white light, the channelled spectra obtained are too complicated for concise description. A very interesting result, however, is the passage of the fringes across the stationary sodium line, when the grating is moved, fore and aft, in a direction normal to its plane. The region of the *D* line is thus alternately dark and bright. The direction of these rays remains unaltered while the illumined strip is shifted horizontally across the ruled space of the second grating. It is sometimes difficult to see the *D* line in the focal plane of the fringes. When homogeneous light is used this fiducial mark is necessarily absent and the cross hairs of the ocular must be supposed to replace it. The shift of the fringes is then equally obvious and sometimes (sodium light) different groups seem to travel in opposite directions while the grating moves in one direction. In case of homogeneous light and two gratings, moreover, the fringes seem to be of minimum size in the conjugate focal plane of the gratings. They increase in size and in turn split up, in focal planes before and behind this.

An insight into these occurrences was finally obtained in observation with homogeneous light, on the spectrometer, by shifting the grating (transmitting) in its own plane, right and left. The fringes in such a case move *bodily* across the field of the telescope, new groups entering on one side for those which leave on the other. These fringes, even if quite distinct, are differently arranged in the coarse and fine series and are frequently accompanied by dark or bright bands. If the ocular is drawn out and set outward from the principal focal plane (at which the slit image is quite sharp) into a different position, the fringes move in a direction opposite to the grating. If the ocular is set inward from the principal focal plane, they move in the same direction as the grating.

This would not be unexpected; but secondary fringes, or something else in the field, seem to remain stationary. Successive fields may be quite different as to arrangement of fine and coarse lines, but all plane gratings exhibit the same phenomena. Thus it is obvious that the fringes of the present paper result from a residual irregularity in the rulings of the grating. Micrometrically, the successive strips of a slit image, however fine, are of unequal intensity. Between these there is diffraction as may be tested by examining the clear glass at the edge of the ruled space.

It is obvious that in the otherwise indistinguishable images of a slit in homogeneous light, however sharp or however narrow, in its own focal plane the nature of its origin still persists and may be detected by observations outside of the principal focal plane. A fine slit is in all cases presupposed and all the phenomena vanish for a wide slit. On the other hand the width of the pencils of parallel rays may be far greater than is necessary to show the strong Fraunhofer lines.

A fuller report of this work has been presented to the Carnegie Institution of Washington, D. C.

THE EFFECT OF PARENTAL ALCOHOLISM (AND CERTAIN OTHER DRUG INTOXICATIONS) UPON THE PROGENY IN THE DOMESTIC FOWL

By Raymond Pearl

BIOLOGICAL LABORATORY, MAINE AGRICULTURAL EXPERIMENT STATION

Received by the Academy, June 7, 1916

The investigation here reported deals with the general problem of the origin and causation of new, heritable variations. That this is one of the most fundamental problems of genetics admits of no doubt. The method by which this general problem is attacked in the present investigation is that of exposing systematically the germ-cells of an animal to something unusual or abnormal in the surrounding conditions, and then analyzing, so far as may be, not only the new heritable variations themselves (provided any such appear), but also the factors which underlie their causation.

The specific problems with which this investigation deals are these:

1. Does the continued administration of ethyl alcohol (or similar narcotic poisons) to the domestic fowl induce precise and specific changes in the germinal material, such as to lead to new, heritable, somatic variations?

2. Failing a specific effect is there a general effect upon the germinal material leading to general degeneracy of the progeny?

3. What in general are the effects upon the soma of the treated individual of the continued administration of such poisons?

4. Are the somatic effects upon the treated individuals of a sort to give any clue to the probable origin, or mechanism of the germinal changes?

The present paper reports, in brief abstract, the results obtained from the beginning of the experiment in September, 1914, to February 1, 1916. A complete report is now in process of publication in another place. In that report the data will be presented in detail, with probable errors, etc.[1]

The foundation stock used in these experiments came from pedigreed strains of two breeds of poultry, Black Hamburgs and Barred Plymouth Rocks. Both of the strains used have been so long pedigree-bred by the writer, and used in such a variety of Mendelian experiments, that they may be regarded as 'reagent strains,' whose genetic behavior under ordinary circumstances may be predicted with a degree of probability amounting practically to complete certainty. Furthermore, the results of crossing these two breeds reciprocally have been thoroughly studied by me.

Three different series of birds were started. To the birds in one series was administrated 95% ethyl alcohol. To those in the second series was administered methyl alcohol, and to those in the third series, ether.

The method followed in these experiments for the administration of the poisons was essentially that which has been used by Stockard,[2] namely the method of inhalation.

The number of treated birds used in the experiments to the date covered in this report is 19. The number of untreated control brothers and sisters is 58.

Results in the treated individuals.—Before entering upon any discussion of the effect of the alcohol treatment on the progeny it seems desirable to examine the effects, both structural and physiological, upon the treated individuals themselves. The main results are summarized in Table I.

The plan of this table is as follows: In the last column of the table a plus sign denotes that, with reference to the particular character discussed, the alcoholists have been favorably affected; a minus sign that they have been unfavorably affected as compared with untreated

TABLE I

SHOWING IN SUMMARY FORM THE EFFECT OF CONTINUED ADMINISTRATION OF ALCOHOL (ETHYL AND METHYL) AND ETHER, BY THE INHALATION METHOD, UPON THE TREATED INDIVIDUALS THEMSELVES

CHARACTER OR QUALITY STUDIED	TREATED INDIVIDUALS	UNTREATED CONTROLS	NET RESULTS ON ALCOHOLISTS
1. Mean number per bird of consecutive days of treatment	344.2	0	
2. Net percentage mortality (to Feb. 1, 1916) exclusive of birds accidentally killed	*0*	41.0	+
3. Mean body weight of females (in gms.)	3266	*2953*	−
4. Mean egg production per bird, 14 months	183.97	180.80	0
5. General activity	Reduced	*Normal*	−
6. Sexual activity	Reduced	*Normal*	−

controls. A zero indicates that no effect of the treatment, one way or the other, has beeen detected.

From these summarized data it is possible to gain a tolerably clear comprehension of the objective happenings in these experiments so far. The treated animals themselves are not conspicuously worse or better than their untreated control sisters or brothers except in respect to mortality rate. This is much lower in the treated than in the con-controls.

Regarding egg production the following details may be mentioned.

The egg production of the treated birds and the untreated controls was entirely normal in respect of its seasonal distribution, as well as in regard to its amount.

There has been *no significant difference* in the egg production of the treated birds and their untreated control sisters, either in the total average number of eggs produced per bird, nor in the seasonal distribution of this production. The only conclusion which can be drawn from the statistically insignificant differences which appear between treated and control birds is that the inhalation treatment has not affected the egg production of the birds, either favorably or adversely.

Results in the F_1 progeny of treated individuals.—The general plan of the matings in 1915 was to breed a treated male of each of the three classes, ethyl, methyl, and ether with (*a*) untreated control females, and with (*b*) treated females of his own class (i.e., ethyl ♂ × ethyl ♀, methyl ♂ × methyl ♀, ether ♂ × ether ♀). In addition to these matings an untreated control male was mated with (*a*) untreated control females (*b*) ethyl females, (*c*) methyl females, and (*d*) ether females.

All of the matings were of the type Black Hamburg ♂ × Barred

Plymouth Rock ♀. There were produced 234 chicks from matings wherein one or both parents were treated.

The following reasoning has been used in devising a numerical expression of the dosage, so far as concerns the progeny. Two germ cells, a sperm and an ovum, unite to form the zygote of each progeny individual. It is proposed to designate as the 'total germ dosage index' the total number of days during which the two gametes making the offspring zygote have been exposed to alcoholic influence while sojourning in the body of the treated individuals.

The total germ dosage index for the F_1 progeny in these experiments ranges from 130 days to 354 days, with the matings for the different substances used well scattered over the range. A summarized statement of the effects on the progeny is given in Table II.

TABLE II

SHOWING IN SUMMARY FORM THE EFFECT OF CONTINUED ADMINISTRATION OF ALCOHOL (ETHYL AND METHYL) AND ETHER BY THE INHALATION METHOD, UPON THE PROGENY

CHARACTER STUDIED	OFFSPRING OF				NET RESULT ON ALCOHOL OFFSPRING
	Treated ♂♂ × untreated ♀♀	Treated ♂♂ × treated ♀♀	All treated parents	Untreated controls	
1. Mean germ dosage index	137.8	299.0	210.35	0	
2. Percentage of infertile eggs (i.e., eggs in which no zygote was formed)	25.2	59.2	41.7	*25.3*	−
3a. Percentage of embryos dying in shell	36.6	*26.9*	33.3	42.2	+
3b. Percentage of fertile eggs (i.e., zygotes) which hatched	63.0	*72.3*	66.7	57.8	+
4. Percentage of all eggs which hatched	47.1	29.4	38.6	*44.4*	−
5. Percentage mortality under 180 days of age	21.1	*10.6*	17.6	36.9	+
6. Percentage mortality over 180 days of age	*5.9*	13.6	10.3	15.3	+
7. Sex ratio: per cent ♂♂	48.9	45.5	47.7	50.0	0
8. Mean hatching weight per bird, males	34.91	*36.97*		34.24	+
9. Mean hatching weight per bird, females	35.04	*37.17*		34.73	+
10. Mean adult weight per bird, males	2669	*2815*		2392	+
11. Mean adult weight per bird, females	2020	*2063*		1928	+
12. Percentage of weak or deformed chicks	0.7	*0*	0.4	1.0	+
13. Abnormalities of Mendelian inheritance	0	0	0	0	0

An examination of this table shows that *out of 12 different characters for which we have exact quantitative data, the offspring of treated parents taken as a group are superior to the offspring of untreated parents in 8 characters.* The offspring of untreated parents are superior to those of the alcoholists in respect of but two characters, and these are characters which are quite highly correlated with each other and really should be

counted as but one single character. Finally with respect to two character groups there is no difference between the alcoholists and the non-alcoholists.

We may evaluate our results in general terms as follows:

1. There is no evidence that *specific* germinal changes have been induced by the alcoholic treatment, at least in those germ cells which produced zygotes.

2. There is no evidence that the germ cells *which produced zygotes* have in any respect been injured or deleteriously affected.

3. The results with poultry are in *apparent* contradiction to the results of Stockard and others with mammals. This contradiction is, however, probably only apparent and not real, paradoxical as such a statement may appear.

4. The results with poultry are in a number of important respects in essentially complete agreement with those of Elderton and Pearson[3] on parental alcoholism in man, and of Nice[4] in mice.

The interpretation of these results which seems to account best for all the facts is that the apparent discrepancy between avian and mammalian results is fundamentally due to a difference in degree of resistance of the germ cells to alcohol. Given the existence of variation in the vigor or resisting power of germ cells, which is certainly a fact, we have the necessary basis for the action of a selective agent. The hypothesis which we wish to suggest is that alcohol acts as such a selective agent upon the germ cells of alcoholized animals, eliminating the weak and permitting the survival of the vigorous and highly resistant.

Detailed evidence in support of this hypothesis is presented in the complete paper. The experiments are being continued.

[1] Some account of this work has already appeared. Cf. Pearl, R., On the effect of continued administration of certain poisons to the domestic fowl, with special reference to the progeny, *Proc. Amer. Phil. Soc.*, **55**, 243–258 (1916).

[2] Cf. for summary and bibliography of earlier papers Stockard, C. R., and Papanicolaou, G., A further analysis of the hereditary transmission of degeneracy and deformities by the descendants of alcoholized mammals, *Amer. Nat.*, **50**, 65–88, 144–177 (1916).

[3] Elderton, E. M. and Pearson, K., A first study of the influence of parental alcoholism on the physique and ability of the offspring, *Eugenics Lab. Mem.*, **10**, 1–46 (1910). (Second edition.)

[4] Nice, L. B., Comparative studies on the effects of alcohol, nicotine, tobacco smoke, and caffeine on white mice. I. Effects on reproduction and growth, *J. Exper. Zöol.*, **13**, 133–153 (1912).

THE EFFECTORS OF SEA-ANEMONES

By G. H. Parker

ZOÖLOGICAL LABORATORY OF THE MUSEUM OF COMPARATIVE ZOÖLOGY AT HARVARD COLLEGE

Received by the Academy, May 6, 1916

Sea-anemones, like other animals, have special sets of organs, effectors, by which they respond to changes in the environment. In these animals there are at least four sets of such organs: the mucous glands, the nematocysts, the cilia, and the muscles.

The mucous glands are unicellular glands found on almost all surfaces of the body and concerned partly in protecting the surface against insult and partly in rendering it sticky whereby the animal as a whole adheres to a rock or other fixed object or foreign bodies adhere to the animal as in the collection of food by the tentacles.

The nematocysts are the well-known organs of defense and offense. They are most numerous and best developed on the tentacles and on the acontia. By their abundant discharge large animals may be stung and driven away and small ones killed and appropriated for food.

Cilia are means of generating currents and of transporting small bodies over surfaces and through tubes. They are best developed on the tentacles, the acontia, and the oesophagus. The oesophageal cilia ordinarily beat outward, but in the presence of food they reverse temporarily and beat inward thus carrying the food into the digestive cavity.

The mucous glands, the nematocysts, and the cilia have all been supposed to be under nervous influence. In all cases their action in reference to the stimulus appears to be strictly local; that is, they become active only over the exact region stimulated. Moreover if sea-anemones are subjected to anesthetics, such as chloretone, or magnesium sulphate, their nervous activitites can be completely abolished but without interfering in the least with the secretion of mucus, the activity of nematocysts or of cilia including their reversal. For these reasons it is concluded that the three types of effectors just mentioned are independent of nervous control and respond to direct stimulation.

Muscles on the other hand are commonly under the influence of nerves, an influence that disappears entirely when the sea-anemone is fully anesthetized. Some muscles, however, such as the longitudinal muscles of the acontia, have been shown to be independent effectors. They are slow in response often requiring a minute or more between the

time of stimulation and that of contraction, and are not influenced by ordinary anesthetics.

The majority of the muscles in a sea-anemone respond quickly under the influence of the nerve-net and enter into a state of enduring contraction (tonicity). This is as characteristic of a small fragment of an animal, provided it contains a nerve-net as well as muscle, as it is of a whole animal. So striking is this excessive tonicity in the muscle of sea-anemones that it has been assumed to be their exclusive function and they have recently been regarded (Jordan) as animals incapable of ordinary reflexes. Such a conclusion, however, seems to be too sweeping.

If a specimen of Metridium is allowed to expand fully and a small piece of meat is placed on its tentacles, the mouth and oesophagus soon open and the sides of the column are marked by a few pronounced vertical grooves. After the food has been swallowed the grooves disappear and the oesophagus closes. The opening of the oesophagus is brought about by the contraction of the transverse muscles of the mesenteries whose action is so precisely associated with the appropriate stimulation of the tentacles that it carries with it all the signs of a reflex. It therefore seems clear that among the muscles in sea-anemones there are not only independent effectors, and tonus muscles associated with nerve-nets, but neuromuscular combinations that exhibit true reflex action.

The detailed paper will be published in the *Journal of Experimental Zoology*.

PRELIMINARY EVIDENCE OF INTERNAL MOTION IN THE SPIRAL NEBULA MESSIER 101

By A. van Maanen

MOUNT WILSON SOLAR OBSERVATORY, CARNEGIE INSTITUTION OF WASHINGTON

Received by the Academy, June 14, 1916

Inasmuch as data for the proper motions of stars, determined by photographic methods, have been rapidly accumulating since the beginning of the century, it may seem strange that the first results for nebulae should have been published only in 1915. It must not be forgotten, however, that photographs of nebulae require much longer exposures, and that, even with the best plates, the measures are more difficult and less accurate, because of the unsymmetrical character of the points and condensations upon which settings must be made, than is the case with the round images of stars. A given point in a nebula may be bisected quite differently on different plates and the measures

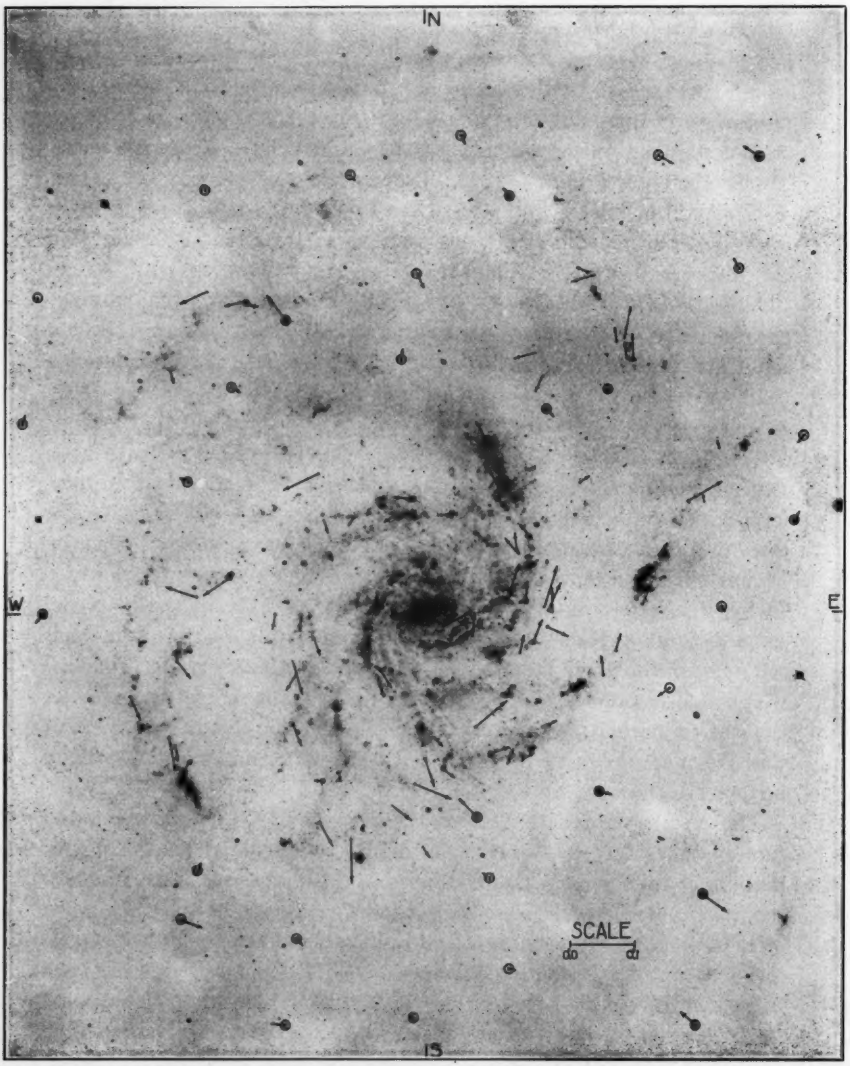
will therefore fail to reveal the proper motion with anything like the precision attainable in a star cluster, for instance. The difficulty of bisection may, however, be largely overcome by measuring corresponding points on different plates in immediate succession.

For such measures the monocular arrangement of the stereocomparator is an admirable instrument, and that it is capable of yielding very accurate results has already been pointed out.[1] When therefore Mr. Ritchey placed at my disposal for measurement two excellent plates of the spiral nebula Messier 101, taken in 1910 and 1915, there was no question but that the stereocomparator was the best instrument for the purpose.

Although the results showed striking evidence of internal motion, the necessity of additional plates was strongly felt. At my request Dr. Curtis kindly placed at my disposal three photographs made with the Crossley reflector of the Lick Observatory, one by Keeler in 1899, one by Perrine in 1908, and one by Curtis himself in 1914.

The pair taken by Mr. Ritchey was completely measured twice, the Lick pairs 1914–1899 and 1908–1899 once each; each pair was measured in four positions, with east, west, north and south, respectively, in the direction of increasing readings of the micrometer screw. On the Mount Wilson plates 87 nebulous points were measured; on the first Lick pair 46, and on the second 69 points, while on all the pairs the same 32 stars were used for comparison purposes. The measures and reductions, which will be published in full in the *Astrophysical Journal*, were made substantially in the manner described in my recent paper on the determination of stellar parallaxes,[2] the principal difference being that the quadratic terms could not here be neglected and were accordingly included in the reductions.

The results showed that to each pair of plates could be given the same weight, and the direct mean of the values found for the proper motions of each point is therefore used in the discussion. The resulting motions, which are those relative to the mean of the 32 comparison stars, are due partly to a motion of translation of the nebula as a whole, and partly to a possible internal motion. The annual motion of translation, which was derived by three different methods of reduction, was found to be: $\mu_\alpha = +\ 0''.005$, $\mu_\delta = -0''.013$. Subtracting these from the total motions, the results are what may be called the internal motions. The accompanying plate shows these mean internal motions for each of the 87 points of the nebula. The individual motions for the comparison stars, which are surrounded by circles are also shown. The scale for the annual motions is given at the bottom of the plate. The



INTERNAL MOTIONS IN MESSIER 101

The arrows indicate the direction and magnitude of the mean annual motions. Their scale (0".1) is indicated on the plate. The scale of the nebula is 1 mm. = 10".5. The comparison stars are enclosed in circles.

density of the center of the nebula has been reduced to show the motions more clearly.

If the results as illustrated on the plate could be taken at their face value, they would certainly indicate a motion of rotation, or possibly motion along the arms of the spiral. Without expressing a final opinion as to the character of the motion, which must be determined by future work, it may be of interest to examine the evidence afforded by the existing material. Comparing the motions with the directions of the branches of the spirals, we find from 52 points in which this direction can be specified with fair accuracy, that the mean divergence of the motions is 7° ± 4° toward the concave side of the spirals.

To discuss the internal motions from the standpoint of rotation, they were analyzed into two components, along and perpendicular to the radius, the latter for convenience being spoken of as the rotational component.

The results are as follows:

78 points have a left-hand motion (N. W. S. E.), only 9 moving right-handedly; 58 points appear to be moving outward, while 28 show motion inward. The rotational motion is the larger in the majority of cases, viz., 63 points. The mean rotational motion is 0".022 left-handed; the mean radial motion is 0".007 outward. The probable reality of the result is indicated by the satisfactory agreement of the pairs of plates, as shown by the following summary.

$\mu_{rot.}$	$\mu_{rad.}$		
+0".021	+0".004	Ritchey	1910 and 1915
+0.032	+0.012	Ritchey	1910 and 1915
+0.012	+0.007	Keeler and Curtis	1899 and 1914
+0.017	+0.006	Keeler and Perrine	1899 and 1908

The measures indicate a small but scarcely reliable decrease of rotational motion with increasing distance from the center, as shown by the following table:

Distance from Center	$\mu_{rot.}$	No. of Points
<3'.1	0".024	19
3.1 to 5.0	0.028	29
5.1 to 7.0	0.014	18
>7.0	0.019	21

The annual rotational component of 0.022 at the mean distance from the center of 5′ corresponds to a rotational period of about 85,000 years. If we knew the parallax of the nebula, and if we could assume that the motions and the distances of the points from the center are

mean values for elliptical orbits, the central mass could be calculated. Even though this assumption may be far from the truth it seems worth while to accept certain more or less hypothetical values of the parallax in order to get any idea of the order of the masses with which we are concerned.

Two estimates as to the parallax can be made, (1) by comparing the average motion of translation of 66 spiral nebulae, as given by Curtis,[3] with those of the stars; (2) by comparing these cross-motions with the observed radial velocities of the few spirals for which such results are known. In the first case we derive a parallax of 0".005; in the second, of 0".0003. The corresponding central masses are in both cases very large, viz., 30,000 and 140,000,000 times that of the sun. Various objections to the acceptance of these results, even as rough guesses can be made, but they are the best we have at the moment. The corresponding orbital motions would be 21 and 345 km./sec. These quantities do not seem absurd if we remember that Wolf by spectroscopic methods found a rotational component of ± 100 km./sec. in Messier 81.[4]

As the detailed results will soon be published I will only mention two more points which seem to confirm the reality of the motions. Mr. Nicholson very kindly spent much time in making check-measures on the plates taken by Mr. Ritchey, both with the stereocomparator and with another measuring machine in which two microscopes were mounted in such a way that they were directed toward corresponding points on the two plates, mounted on the same plate-carrier and moved by the same micrometer screw. His measures give satisfactory confirmation of my own results. Further, I have measured two plates of Messier 81, taken by Mr. Ritchey in 1910 and 1916, which show motion similar to that found above for Messier 101. It seems hardly necessary to suggest the importance of internal motions, such as are indicated here, in connection with the Chamberlin-Moulton hypothesis as to the origin of spiral nebulae, and it is to be hoped that material will soon be available for a fuller discussion.

[1] *Astronomical Journal*, 27, 140 (1912).
[2] *Mt. Wilson Contr.*, No. 111, 4 seq., 1916.
[3] *Publications Astronomical Society Pacific*, 27, 216 (1915).
[4] *Vierteljahrsschrift Astronomischen Gesellschaft*, 49, 162 (1914).

THE EXPLORATION OF THE PACIFIC

By W. M. Davis

DEPARTMENT OF GEOLOGY AND GEOGRAPHY, HARVARD UNIVERSITY

Read before the Academy, April 17, 1916. Received May 25, 1916

A voyage across the Pacific, such as the Shaler Memorial voyage that I made two years ago, gives a traveller time on the free days between groups of islands to think of many problems, old and new; and thoughts of unsolved problems tend to run on into dreams about their possible solution. Even after returning home, the dreams continue in intervals of rest between spells of work on the results of the voyage; for just as the unrecorded facts that one so often finds while in the broad ocean impress their observer with the great volume of work still to be done, so the records of earlier work which one reads in a home library too frequently disappoint him by their incompleteness, their insufficience; thus at home and abroad, the wish of a fuller accomplishment is repeatedly present.

The insufficience of earlier work and hence the abundant opportunity for new work was first borne in upon me in connection with the old problem of coral reefs, to the investigation of which my own voyage was directed. This does not mean that a later traveller is ungrateful for what his predecessors have done, but that he need not carry gratitude to the point of being thankful to them for having left so rich and ripe a harvest to be gathered by their successors. Nor would a reference to the insufficience of earlier work be just, if the insufficiency concerned matters or methods unknown in earlier years. But when their insufficiency concerns truths so well established as the geological principle of unconformity and the physiographic principle of embayed shores, known and understood, one for a century, the other for half a century past, and directly pertinent in the problem of coral reefs, one must wonder in how many other directions the work thus far performed in the Pacific is incomplete. A student of coral reefs today cannot avoid regret at the repeated failure of earlier observers to note matters so manifest as the embayed shore lines of the central islands within barrier reefs, and the unconformable contacts of many fringing and elevated reefs with their deeply eroded foundations, inasmuch as these simple and evident relations immediately demonstrate submergence in association with reef formation, and thus contribute valiantly to the solution of the old coral-reef problem. With the regretful discovery of such oversights with respect to one's own special subject in the records of the most famous Pacific expeditions, one cannot avoid suspecting the occurrence

of similar oversights in other subjects, and one must thereupon experience a great wish to see thorough and comprehensive work instituted in all subjects with the least possible delay.

For example, following the belief of the eminent Austrian geologist, Suess, regarding changes of ocean level, two responsible students of coral reefs, who must certainly have been regarded as competent investigators by the institutions that sent them forth on their travels, have independently announced that certain high-standing atolls owe their altitude to a subsidence of the ocean—the entire ocean—while these atolls, little specks in the vast ocean, stood fixed: yet it may be easily shown that that explanation is preposterously inadequate to account for the total facts. Hence, if inadequate treatment has been given to so fundamental a problem as this, is it too much to say that the more recondite problems of the Pacific cannot be solved by the methods heretofore in use? Even subjects of so large dimensions as the circulation of the atmosphere over the Pacific and of the Pacific waters in their basin are known to us only in the most general way; like many other subjects, our knowledge of them comes from records made by observers little trained in special fields. It cannot be doubted that large rewards will follow from exploration systematically carried on by trained specialists.

Adventurous voyages of discovery sufficed in the eighteenth century, when the method of exploration may be described as discontinuous and local. Less adventurous and more scientific voyages of investigation in the nineteenth century made brave attack on many problems by a method that may be described as continuous and linear. Now, so exacting have the demands of science become that nothing less than an areal survey of the Pacific will satisfy them; that is, a survey in which all the islands shall be included, and in which the successive routes of linear observation on the ocean shall be so closely interwoven that, like the work of the magnetic survey of the Pacific by the Carnegie Institution of Washington, the results gained may be reasonably regarded as applicable to the intermediate spaces.

The exploration of the Pacific should not only be continuous in the areal sense, but continued in the time sense. Many problems will call for 'one voyage to learn,' however careful the preparation before setting out. Exploration should therefore be continued through a number of years under one administration, so that the voyaging staff may gain in the early voyages the expertness needed for the solution of their difficult tasks in the later voyages. Many returns must be made to critical points, where first observations regarding air, water, land, or life

will hardly do more than open up the more complicated subjects for investigation; for Pacific exploration should not only be continuous in the areal sense, and continued in the time sense, but comprehensive in every sense. It should reach from the depths of the sea to the heights of the air; from the cold waters of the North, narrowly limited in their connection with the Arctic ocean, across the torrid zone, to the cold waters of the South, broadly continuous with the Antarctic; from the little broken shores of continental America to the repeatedly interrupted shores of half-drowned Australasia; it should include all the islands of the open ocean, and all forms of life from bacteria to man.

The exploration of the Pacific is not a new theme. The voyages of Magellan, Cook, and others gave it early fame. The United States Exploring Expedition under Captain, later Commodore, Wilkes made a great advance 80 years ago. Forty years afterwards, the 'Challenger' did forty-years better; and yet we now learn from the refined studies of Norwegian hydrographers that the errors of certain instruments in the 'Challenger' outfit were greater than the variations of fact which those instruments were intended to measure. An interesting but sporadic and not long-lasting effort at Pacific exploration was made some 40 years ago by the Museum Godeffroy, founded at Hamburg by a firm of merchants: the Dutch Government has carried on scientific investigations of many kinds in its East Indian possessions, bordering the Pacific; much excellent work is now in process at the Bishop Museum, centrally located at Honolulu, where the great galleries of collections open to the public are backed by a large building devoted to research laboratories. The Hawaiian Islands seem, indeed, to be a center of inspiration on our subject, for a resident there, Prof. W. A. Bryan, elaborated a general scheme of Pacific exploration several years ago, and a recent visitor, Prof. R. A. Daly, has lately discussed the same problem; and in Hawaii as well as in the Philippines our governmental bureaus are prosecuting cartographic and other surveys. Specialists cross the Pacific from time to time on particular quests; the following papers will tell something of such work by Messrs. Briggs, Iddings, Pilsbry and Campbell. But the Pacific is vast. Discontinuous, local or linear, individual work, economically conducted, cannot, however excellent, compass the immense extent and the infinite variety of that great water hemisphere. Thorough-going Pacific exploration will demand most munificent support.

It has been urged by some of those with whom I have talked on the financial side of the scheme that the present troublous times are not propitious for its launching. True; and we are not launching it now; we are only laying its keel. But these troublous times cannot last in-

definitely; they will be past before we have had time—two, three, or four years—for the deliberate development of our plans. It is therefore with no idea of immediate action that the dreams of certain spare hours and days in the last two years have here been written down in brief outline; nor is it with the least intention or expectation on my part of sharing in the work of exploration, if the dreams are realized, that the outline is here presented. The work must be done by men of middle age or less, and they must be selected and directed by whomsoever brings the dreams into execution. If some antipodal Croesus rise to the occasion, the plan is his and welcome; but I believe that, if the plan is carried out at all, it will be by Americans, to whom the scientific conquest of the Pacific may make strong appeal. Even after the war is over, European munificence, if any of it survive, will be heavily burdened with home duties: and while the war lasts, even American capital may be so largely invested in commercial enterprises that little of it will be diverted to science: but when peace comes it is by no means beyond the limit of possibilities that our plan may arouse the interest of an American patron; for America's outlook upon the Pacific is large. Hence, in scientific as well as in national affairs, preparedness may well be our motto; and the first step in preparedness is—not a precipitate plunge into uncorrelated action—but the careful consideration of a comprehensive plan.

All that can be accomplished today is, to summarize a few of the broad problems that have been opened but not closed in the greatest of the world's oceans; and this will be done by the following speakers, who have generously responded to my appeal. Each one will touch briefly on certain topics—merely a few of many—yet representative, each one of all. I shall at some later time ask again if the whole subject, of which a few parts now are to be set before us, is not worthy of further consideration by the Academy, in the hope—not a vain hope, I believe—that the preparation of a well developed plan of investigation may be the prelude to a grand undertaking and a superb accomplishment.

THE IMPORTANCE OF GRAVITY OBSERVATIONS AT SEA ON THE PACIFIC

By John F. Hayford

COLLEGE OF ENGINEERING, NORTHWESTERN UNIVERSITY

Read before the Academy, April 17, 1916. Received, May 24, 1916

The time is ripe for decided advances in our knowledge of geodesy and geology to be made by a study of observed values of gravity. Observations for this purpose are needed at sea, especially—rather than

on land. On the seas a given amount of observing will produce most progress if that observing is done on the Pacific.

In geodesy, gravity determinations furnish the most powerful, the most accurate, known method of measuring the flattening of the earth, and thereby furnish the most severe, and therefore the most valuable, single test of the reliability of conclusions drawn from the deflections of the vertical which are put in evidence by triangulation and astronomic observations.

So also when conclusions have been reached as to the completeness and location of isostatic compensation on the basis of observed deflections of the vertical the most valuable single test of those conclusions is furnished by observed values of gravity.

The geodetic evidence as to the completeness and location of isostatic compensation furnishes effective tests of the validity of an important group of the fundamental ideas of geology. These tests are being applied by the geologists more frequently and more energetically each year.

Moreover, it has recently been shown by Mr. William Bowie,[1] that after observed values of gravity have been corrected for isostatic compensation the remaining anomalies, indicating outstanding excesses or deficiencies of density beneath the surface of the earth are, in some cases at least, related to the geological history of the region. It is probable that in due time geodesy will, in this line, furnish additional help to geology.

In general geodesy furnishes the most powerful known means of investigating the distribution of density beneath the earth's surface, to a moderate depth, say 200 miles. Hence any geological premise which depends on assumptions as to the distribution of densities, within that 200 mile zone—and there are many such premises—finds a severe test in the geodetic evidence. So geodesy may, and will, help the progress of geology.

Suppose it is granted that it is important to secure additional gravity observations. Why is it especially important to secure the additional observations at sea rather than on land?

Good determinations of gravity have already been made at 3000 widely scattered stations on the one-quarter of the earth's surface which is land. No reliable observations of the necessary degree of accuracy have been made on the three-fourths of the earth's surface which is covered by water.

As soon as it becomes possible to determine gravity satisfactorily on a moving ship at sea, it will be possible to secure observations so

rapidly at very widely scattered stations that the new observations made within a single year may furnish a more accurate value of the flattening of the earth than has yet been obtained from all the work of the past. Let us make this more definite and concrete. Assume that it becomes possible to take a series of observations on a moving ship which will determine the force of gravitation at the point of observation with a probable error of ±0.008 dyne (about 4 times the probable error of a land determination). Past experience indicates that the probable error in one such result due to all causes, including the anomalous part of the distribution of densities beneath the surface, will be less than ±0.020 dyne. Six hundred such observations could be secured in a single year scattered from latitude 60°N. to latitude 60°S. From these observations alone the value of the polar flattening of the earth could be computed more accurately than it has yet been computed.

Of course if the best observations that can be made at sea are of less accuracy than *this* their value will be less.

I am making this statement on the assumption that all such observations would be corrected for topography and isostatic compensation by the method now in use in the Coast and Geodetic Survey. Such corrections are essential to reliability and serve to increase greatly the accuracy of the computation of the flattening. I have just compared a very recent computation[2] by F. R. Helmert of the flattening of the earth with an older computation[3] of the flattening by William Bowie. Both used gravity determinations. Helmert used 700 widely scattered selected observations from among the 3000 available over the whole world. Bowie used 122 out of the 124 gravity observations in the United States alone. Helmert made no corrections for topography and isostatic compensation. Bowie applied such corrections. I am convinced from a study of the evidence, including the evidence of systematic errors, that Bowie's value of the flattening derived from 122 gravity observations in a small region is more accurate and reliable than Helmert's value from 700 carefully selected and widely scattered observations.

Observed values of gravity in the United States after correction for topography and isostatic compensation show no relation to the topography. On the other hand without such corrections, as Helmert's recent investigation again shows clearly, observations along the coasts stand in a class by themselves, observations in low interior regions in another class, those in mountainous regions in another class, and those on small oceanic islands in still another class, and each class is subject to its own peculiar systematic errors which are large.

It should be evident that it is extremely desirable to extend to the open oceans the proof, which is now conclusive for land areas, that the application of corrections for topography and compensation makes the corrected results independent of topographic effects and eliminates a large part of the systematic error otherwise inherent in the results. I am confident that good observations at sea will promptly furnish such a proof. They would certainly give a very severe and therefore very valuable test of the conclusions as to isostasy which have been drawn from the observations on land.

It is obviously important to determine as well as may be the prevailing depth at which masses of abnormal density lie. Are they ordinarily within 10 miles of the surface, or are they as frequently more than 40 miles down? The nearer to the surface such an abnormal mass lies the more rapid will be the space-change of gravity as an observer approaches, passes over, and recedes from the region on the surface which lies above the abnormal mass. For this purpose, therefore, for indicating the approximate depth of abnormal masses, it should be evident that closely spaced continuous lines of gravity observations at sea such as could be easily secured would be much more effective than are stations on land under ordinary conditions.

To what is the permanency, or semi-permanency, of the great oceanic depressions due? Adequate gravity observations at sea would establish conclusively the extent to which the rocks underlying the oceans are more dense than those under the land, and thus furnish a conclusive partial answer to the question.

The present indications, from a few gravity stations on such islands, is that gravity is in excess on oceanic islands, such as the Hawaiian Islands, where vulcanism is active. How far does said excess extend out to sea? Observations of gravity at sea would answer that question and in doing so might contribute much to our knowledge of the nature and cause of vulcanism.

What is the nature of the deep troughs that occur at various places in the oceans and which show a suggestive tendency to be located near and parallel to an elongated land area or a mountain chain? Gravity observations at sea may throw a light upon this question by showing the density of the rocks below such troughs.

I have indicated why I believe it to be especially important to secure gravity observations at sea. If such observations are to be made, why is the Pacific the ocean upon which they will be most effective?

Of course it occurs to one at once that the Pacific is the greatest ocean and that therefore the largest blanks in which there are now no gravity

observations are there. That will on examination prove to be a more weighty consideration in favor of the Pacific than appears at the first glance.

Two other considerations also combine with this to indicate strongly that the Pacific is the most effective place to make gravity observations at sea in order to advance our knowledge of geodesy and geology.

First, it is important to get observations at sea so far from any continent as to be certainly free from any continental effect.

It is difficult to get 2000 miles from all continents on the Atlantic. That distance is not sufficient. On the Pacific there is a considerable area 3000 miles from any continent. The Pacific ocean occupies the water hemisphere.

The second consideration in favor of the Pacific is that it offers an unequaled variety of special opportunities to study special questions under extreme and contrasting conditions. In it there are small oceanic islands of volcanic origin far from land, and equally small oceanic islands far from land apparently not of volcanic origin. There are several troughs more than 8000 meters deep lying adjacent to large islands in some cases and in others far from any but very small islands. There are flat bottomed areas of more than 6000 meters depth of various sizes, some covering several square degrees, and in various relations to land. There are several areas of less than 200 meters depth, some of which are far from any land, except very small islands. No other ocean offers an equal variety of equally favorable opportunities for special studies based on gravity observations at sea.

Adequate observations of gravity, at sea, on the Pacific Ocean would contribute greatly to progress in geodesy and geology.

[1] Special Publication No. 10 of the Coast and Goedetic Survey, pp. 113–117 and Special Publication No. 12, pp. 18–21. Both these publicaflons are under the title Effect of Tópography and Isostatic Compensation upon the Intensity of Gravity.

[2] "Neue Formeln für den Verlauf der Schwerkraft im Meeresniveau beim Festlande," von F. R. Helmert; Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften, 1915, XLI, Gesamtsitsung vom 21 Oktober.

[3] Special Publication No. 12 of the Coast and Geodetic Survey, pp. 24–26.

A NEW METHOD OF MEASURING THE ACCELERATION OF GRAVITY AT SEA

By Lyman J. Briggs

BUREAU OF PLANT INDUSTRY, WASHINGTON, D. C.

Read before the Academy, April 17, 1916. Received June 19, 1916

Introduction.—The desirability of more extensive and more accurate measurements of the acceleration of gravity at sea has recently been emphasized by Bauer[1], Bowie[2], and Hayford.[3] Such measurements are essential in the accurate determination of the figure of the earth. Ocean gravity measurements now available are due almost wholly to Hecker,[4] who employed the mercurial barometer-hypsometer method in an elaborate and extended series of measurements in the Atlantic, Pacific, and Indian oceans. In this method the outstanding difference at a given station between the atmospheric pressure as computed from the boiling point of water and directly observed with the mercurial barometer is attributed to the difference in the gravitational force acting on the mercurial column at the given station and at the standard station (Lat. 45°, sea level).

There are certain difficulties inherent in the barometer-hypsometer method which greatly lessen its usefulness. (1) The atmospheric pressure must be determined in absolute measure by each method in order that the determinations may be comparable, so that systematic errors are serious; (2) the boiling point determinations must be carried out with the highest degree of refinement in order to secure even moderate precision in the determination of g. The observed boiling points are not simply differential measurements. The true temperature interval between the melting point of ice and the observed boiling point must be known in terms of the hydrogen-scale before reference can be made to vapor pressure tables for the determination of the atmospheric pressure.

An independent measure of the accuracy attainable in the determination of atmospheric pressure by the boiling point method is afforded by measurements of the 'fundamental interval' of standard mercurial thermometers. Waidner and Dickinson[5] found in a study of the standard mercurial thermometers of the Bureau of Standards that the fundamental intervals varied through a range of 0.015C. during the ten-day period covered by the measurements. This variation they attribute in part to the sticking of the meniscus with resulting variation in the capillary pressure.

The probable error of a fundamental interval determination in Waid-

ner and Dickinson's measurements is about ±0°.003C. The measurements were carried out with a refinement which is probably unattainable at sea, and may be taken to represent the limit of accuracy attainable in such determinations on board ship. We may now consider the effect of such an error in the determination of g. A probable error of ±0°.003C. in the true boiling point temperature would correspond in atmospheric pressure to ±0.083 mm. of mercury and to a probable error of ±0.11 cm. per sec. per sec. in the value of g. The uncertainties due to the boiling-point measurements alone under the most favorable conditions would, therefore, result in a probable error of 1 part in 10,000 in the value of g. This is 13 times the limit of error set by Hayford for ocean gravity measurements. To this must be added the errors in the barometric observations. Errors in the boiling-point temperature alone would justify Bowie's statement that Hecker's gravity measurements at sea "are subject to uncertainties as large as the largest of the new-method anomalies of gravity in the United States, that is, between 0.05 dyne and 0.10 dyne."

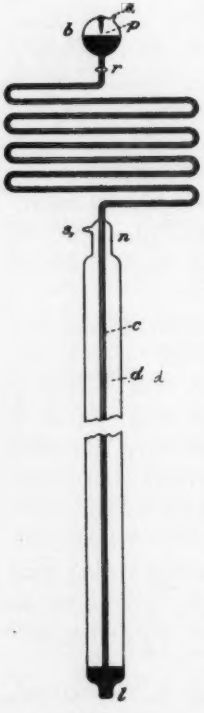


FIG. 1.

Description of the New Method.—The new method of measuring g at sea described in this paper is based upon the change in height of a barometric column sealed off from communication with the atmosphere.[6] All boiling-point determinations are thus avoided. The apparatus is kept at constant temperature in a bath of melting ice. The determination only of the position of the upper end of the barometric column is necessary. The design of the instrument is such that in making this setting the enclosed gas mass is automatically reduced to a constant volume; and since temperature is constant, the measurements are made at constant pressure.

A sketch of the glass part of the apparatus is shown in figure 1. The mercurial column is contained in the capillary c (bore 0.6–0.7 mm.) the lower end of which opens beneath mercury in the bottom of the gas chamber d. This capillary is sealed to the wall of the gas-chamber where it passes through the upper end. The upper part of the capillary is bent into a flexible zig-zag, and expands into the spherical bulb b (diameter 2 cm.) The bulb contains a fixed iron point p sealed to the inside of the

bulb by means of an inserted platinum wire and extending vertically downward, so that the point is approximately at the center of the bulb. The length of the mercurial column is about 74 cm.

The flexible capillary permits a slight vertical movement of the observing bulb with respect to the gas chamber. This movement is determined by a micrometer screw of 1 mm. pitch which controls the motion of a carriage in which the observing bulb is rigidly mounted. The carriage slides on parallel rods mounted on a base which is rigidly cemented to the neck *n* of the gas chamber, so that the position of the bulb relative to the gas chamber is definitely determined by the screw.

After the apparatus has been thoroughly cleaned and dried, pure mercury is introduced in excess of the amount required to fill the capillary and observing bulb. The apparatus is then highly evacuated through s_1 and s_2 and the observing bulb sealed at s_1. Dry nitrogen is introduced into the gas chamber in excess of atmospheric pressure through a stop-cock temporarily sealed to the chamber at s_2. The micrometer head is then mounted, the protective casing of the gas chamber adjusted, and the whole apparatus placed in the ice-tank in a vertical position and surrounded with melting ice. A large U-type vacuum manometer is connected with the stop-cock of the gas chamber and the air exhausted from the connecting tube. After temperature equilibrium is attained, the stop-cock of the gas chamber is opened and the pressure of the nitrogen slowly reduced until the mercury stands in contact with the fixed point in the observing bulb when the carriage is near the middle of its range. The pressure in the gas chamber at this time is determined by reading the difference in level of the mercury surfaces in the manometer by means of a cathetometer. The observed pressure, corrected to mercury at 0°C., gives the difference in height of the two mercury surfaces in the gravity apparatus. The length of the column, less the scale reading, gives the 'constant' of the instrument, which is added to subsequent micrometer readings to obtain the true height of the mercurial column.

When the above determination is made, the gas chamber connection is sealed off with a blow-pipe. The apparatus is now perfectly gas-tight, since it is entirely free from ground joints, stop-cocks and sealed-in platinum connections, and the original openings at s_1 and s_2 have both been sealed with the blow-pipe. The apparatus is also readily transportable, since at ordinary temperatures the pressure in the gas chamber is sufficient to fill the observing bulb with mercury. It can then be tipped in any position.

In observing, the apparatus is supported in a vertical position in

the cork-covered ice tank and completely covered with fragments of melting ice 1 to 3 cm. in thickness, dropped loosely into the tank. The fixed point is observed through a glass tube introduced through the ice and is illuminated through a similar tube on the opposite side. The observing tube is equipped with a low power lens. The ice tank is 25 cm. in diameter and is covered with cork lagging 7.5 cm. thick.

At sea the tank is swung from gimbals, the outer gimbal ring being suspended from 4 spiral steel springs hung from the four corners of a frame of 1-inch galvanized pipe, the four legs of which are screwed to the deck. Movable weights on the bottom of the tank serve to adjust the apparatus to a vertical position, which is determined by means of a sensitive level on the head of the instrument. When the apparatus is mounted on the open deck, an effective wind shield is essential, the supports of which must be entirely independent of the apparatus.

Theory.—Let us determine through a series of observations the height of the mercurial column at some station where g is accurately known. Let us now consider the apparatus to be transported to another station, where g is greater. The mercurial column will be depressed, compressing the gas. The observing bulb is accordingly lowered until the fixed point is again in grazing contact with the mercury surface. The volume of mercury in the bulb and capillary is now the same as at the first station, since the slight flexure of the capillary produces no appreciable change in its volume. Therefore the volume of mercury in the gas chamber is also the same as in the first observation. In other words, the volume of the gas is constant when the upper mercury surface is in grazing contact with the fixed point, and is independent of the position of the bulb. Since the temperature is constant, the pressure p of the gas is always the same at the time of taking an observation. We have then

$$p = \rho g h = g_1 h_1 \tag{1}$$

in which g and g_1 represent the acceleration of gravity at the two stations, h and h_1 the corresponding heights of the mercury column, and ρ the density of mercury. The latter is constant, since the mercury is always at the temperature of melting ice. Equation (1) then reduces to

$$g / g_1 = h_1 / h \tag{2}$$

or, the height of the column is inversely proportional to the acceleration of gravity. If the height h is measured at some station where g is known, the acceleration of gravity at any other station may therefore be determined simply by measuring h_1.

Let $h - h_1 = \Delta h$ represent the difference in the height of the column at the two stations. On substituting this value of h_1 in equation (2) we have

$$g / g_1 = 1 - \Delta h / h \qquad (3)$$

in which Δh represents simply the difference of the micrometer readings at the two stations, and does not involve the absolute height of the column. Since h is at least 200 times as large as Δh, the absolute height of the column does not, in this method, need to be determined with great precision.

Discussion of errors.—The apparatus was designed with the object of obtaining, if possible, an accuracy of 1 part in 100,000 in the measurements. The discussion of the various sources of error will therefore be made on this basis.

Temperature variation.—During the measurements the whole apparatus was surrounded with melting ice, so that the effect of slight variations in temperature need be considered only in connection with the temperature of the gas. Artificial ice was employed and all ice that was not perfectly clear and crystalline was discarded. The purity was systematically checked by measuring the electrical conductivity of the tap water. The impurities were never sufficient to depress the theoretical freezing point more than 0°.001C., whereas a variation of ±0°.003C. would be necessary to produce a change of 1 in 100,000 in the gas pressure. The drip water was allowed to escape from the ice tank through a small trap near the bottom in order to insure the ice extending below the bottom of the gas chamber.[7]

Error in setting point in contact with mercury surface.—Lord Rayleigh[8] found in his investigations with the micromanometer that the fixed ground glass point could be set in contact with the mercury surface with an error not exceeding ±0.0015 mm. An accuracy of 1 part in 100,000 in gravity observations necessitates a probable error in the micrometer observations not greater than ±0.008 mm., which is readily attainable from a series of readings at sea under favorable conditions. The writer has found that in measurements at sea a metallic point is superior to a glass point, due to the fact that the latter becomes electrified through the motion of the mercury surface and this influences the readings.

Error in the determination of the instrument constant.—Reference to equation (3) will show that since the height of the column is at least 200 times the total range (Δh), the uncertainty in h may be 100 times that permissible in Δh. The constant of the instrument (i.e., the vertical distance from the lower mercury surface to the zero on the scale)

therefore does not need to be known with an accuracy greater than 0.5 mm. Determinationsof the constant agreeing to 0.1 mm. can be obtained with the auxiliary manometer to which reference has already been made.

Thermal hysteresis of the glass envelop.—This phenomenon is generally recognized in precision thermometry, and results in the so-called depression of the zero. This effect would tend to introduce an uncertainty in the determination of the difference in the acceleration of gravity at two stations where it is necessary to remove the apparatus from the ice in proceeding from one station to the other. A slight change of this kind (2 parts in 100,000) was observed at Balboa, following the dismantling of the apparatus and its transportation across the isthmus by rail, owing to the slides in the canal. These instruments were made of German glass. Waidner and Dickinson[9] state that thermometers made of the best boro-silicate glass show a depression of the zero of 0°.03. This would correspond to a change of 1 part in 200,000 in the volume of the bulb. Consequently if boro-silicate glass were used in the construction of the glass parts of the apparatus, the error due to thermal hysteresis would fall within the limit set in this discussion.

Disturbances arising from the motion of the ship.—Three classes of disturbances are encountered on board ship:

1. *Tremors*, due to the engines or auxiliary machinery or to the impact of waves. The effect of such disturbances can be greatly reduced by suspending the ice tank from spiral springs. This form of support does not eliminate all vibration. Slight tremors however appear to be advantageous rather than otherwise, as they help to bring the mercury surface to its true position in the bulb.

2. *Horizontal translation*, due chiefly to rolling, which tends to swing the apparatus from its vertical position. Such translations produce two effects which can be made to counteract each other to some extent. (a) The vertical component of the column is shortened by an amount proportional to the sine of the deflection from the vertical. Such deflections therefore tend to increase the length of the column. (b) The centrifugal force resulting from the deflection tends to depress the column when the latter is mainly below the point of support. This can be eliminated by mounting the column so that it is bisected by the gimbal plane. By suitably adjusting the position of the column, the two effects can thus be made to compensate in part.[10]

3. *Vertical motion*, due to rolling and pitching or to the rise and fall of the ship as a whole in a heavy sea. This is the most serious of all the disturbances to contend with, for the motion is accelerated, and is superimposed on the gravitational acceleration. Extensive damping through the use of the capillary column has been employed in the apparatus here described to reduce

the effect of vertical motion. The expansion of the top of the column into a bulb having a cross-sectional area 500–800 times that of the capillary reduces proportionally the change in level in comparison with the actual movement of mercury in the capillary. The rate of change in the acceleration of gravity with latitude is so slight that extensive damping is permissible from this standpoint, although the time required to secure an observation is of course correspondingly increased.

Correction for the course and speed of the ship.—Eötvös[11] has shown the necessity of applying a correction for the easterly or westerly motion of the ship, due to the fact that the ship's motion modifies the angular velocity of revolution of the apparatus about the earth's axis. The centrifugal force acting on the mercurial column when on board a ship moving east or west is therefore not the same as when the ship is at rest or moving north or south. The correction may be as great as 1 part in 10,000, but can be accurately computed if the course, speed, and approximate latitude of the ship are known.

The probable error of the observations.—In 1914, observations were made from Sydney, Australia, to San Francisco by way of Wellington, N. Z.; and in 1915, two instruments were taken from New York to San Francisco via Panama. The following table shows the results of gravity determinations on board ship in various harbors during the two voyages, and where pendulum observations are available near the stations they are appended for comparison. The 1915 observations show a mean probable error in the harbor determinations of 13 parts in 1,000,000. The readings of the two instruments at sea were for the most part as consistent as at the harbor stations. The ocean measurements will be discussed in a later paper.

The method is by no means to be considered as perfected. One of the instruments used in 1915 gave results consistently lower than the other. This indicates a systematic error which must be located. It is also highly desirable that new instruments constructed of boro-silicate glass should be carried over the same course several times with different sea conditions in order to determine whether systematic errors are introduced into measurements made on a rough sea.

Acknowledgments.—In the first apparatus constructed, the gas chamber and capillary were made of steel. This apparatus developed a leak on the voyage to Sydney, and repeated attempts to repair it in the Fiji Islands and at Sydney met with failure. This disappointment was however more than offset by the kindness of Dr. J. A. Pollock, Professor of Physics in the University of Sydney, who placed the facilities of his laboratory and the services of his glass-blower and mechanician

TABLE I.

GRAVITY DETERMINATIONS ON BOARD SHIP AT HARBOR STATIONS

1914

With San Francisco as base,

	cm./(sec.)²
g at Wellington, N. Z.,	
observed, instrument No. 1	980.316
by pendulum (Wright)	980.292
	+0.024
g at Sydney, N. S. W.,	
observed, instrument No. 1	979.68
by pendulum (Smithsonian tables)	979.69
	−0.01

1915

With New York as base,

g at Colon, Panama,	
observed, instrument No. 2	978.196
instrument No. 5	978.236
	978.216±0.013
g at Balboa, Panama,	
observed, instrument No. 2	978.175
instrument No. 5	978.195
	978.185±0.007
g at San Francisco,	
observed, instrument No. 2	979.940
instrument No. 5	979.995
	979.968±0.019
by pendulum (Smithsonian tables)	979.98
	0.012±0.019

at my disposal in the construction of a new apparatus of glass, which was used on the return voyage.

I am also deeply indebted to Dr. C. G. Abbot, Director of the Astrophysical Observatory of the Smithsonian Institution, for his assistance in connection with the steel apparatus, and for valued suggestions regarding the construction of the later form.

Lieut. R. S. Wright, R.E., has kindly supplied the value of g at Wellington from his unpublished pendulum observations made in connection with the Scott Antarctic expedition.

The voyage in 1914 was made with grants from the Australian and New Zealand governments, in connection with the Australian meeting of the British Association for the Advancement of Science. I desire to express my obligation to these governments and also to the American Associ-

ation for the Advancement of Science for a grant used in connection with the 1915 measurements.

[1] Bauer, L. A., On gravity determinations at sea, *Amer. J. Sci.*, 31, 1 (1911). Hecker's remarks on ocean gravity observations, *Amer. J. Sci.* 33, 245 (1912).

[2] Bowie, Wm., Isostasy and the size and shape of the earth, *Science*, 39, 705 (1914).

[3] Hayford, J. F., these PROCEEDINGS, 2, 394 (1916).

[4] For a description of the apparatus employed by Hecker and a summary of his ocean measurements, see Hecker, O., Bestimmung der Schwerkraft auf dem Schwarzen Meere und, an dessen Kuste sowie neue Ausgleichung der Schwerkraftmessungen aud dem Atlantischen, Indischen, und Groszen Ozean, *Zentralbur. Internat. Erdmessung, Veroffentlichungen Berlin*, N. F., Nr. 20 (1910).

[5] *Washington, D. C., Bull. Bur. Standards*, 3, 663 (1907).

[6] This principle has already been employed by Mascart, [*Paris, C. R. Acad. Sci.*, 95, 631, 1882], who used a sealed-off barometer of the U-tube type. The temperature was not controlled, and volume and pressure were both variable. The gas volume had therefore to be measured, the pressure corresponding to the observed temperature and volume calculated, and finally reduced to standard conditions.

[7] This procedure now seems undesirable, since Pernet has observed a departure of 0°.01C. from the true zero when the bulb of a thermometer is surrounded by artificial ice freely drained, due to the ice being undercooled. (See Pernet, J., Sur les moyens d'eliminer l'influence de la variation des points fixes des thermometres a mercure. *Travaux. Bur., International des Poids et Mesures*, 1881, second partie.) In the writer's measurements, ice was added in small quantities at frequent intervals so as to keep the tanks completely filled, and remained in the apparatus for several hours before melting sufficiently to sink to the level of the gas chamber. It is consequently doubtful whether the effect observed by Pernet influenced the measurements, but it is a possible source of error which can be avoided by keeping the interstitial spaces filled with water.

[8] *Phil. Trans. R. Soc., London*, 196, 205 (1901).

[9] *Loc. cit.*

[10] The above procedure was not followed strictly in the trials of the apparatus which have so far been made at sea, due to the difficulty of following the observing tube with the eye when the ship is rolling in a heavy sea. Experience shows however that such an arrangement is necessary if observations are to be made in rough weather, and the difficulty in observing can apparently be met by a modification of the viewing apparatus.

[11] See Hecker, *loq. cit.*

THE PROBLEM OF CONTINENTAL FRACTURING AND DIASTROPHISM IN OCEANICA

By Charles Schuchert

DEPARTMENT OF GEOLOGY, YALE UNIVERSITY

Read before the Academy, April 17, 1916. Received, June 1, 1916

Paleogeographic studies during the past thirty years have been developing the hypothesis that the ancient continental platforms were arranged latitudinally rather than longitudinally as they are now, and, further, that their areal extent, including their emergent and submerged portions, was greater than at present. It appears that vast land-masses have been fractured, broken up, and more or less permanently taken

possession of by the oceans, a history which none exhibits better than the Australia-New Zealand region.

We have learned from the several deep-sea expeditions something of the rare and strange life of the oceanic abysses. An analysis of these organisms shows that no Paleozoic forms occur among them, and very little of the life indeed is ancestrally traceable even to the stocks of Triassic times. It is with the Jurassic and later life that the organisms of the abysses have their affinities. This seems to indicate that the oceans have been progressively deepened only since the Triassic. As one of the most marked crustal deformations, however, began in the Coal Measures of the Paleozoic and continued, though with pauses, well into the Triassic, it therefore appears that the oceans have been progressively enlarged and deepened ever since Permian time. This is in keeping with the theory that the earth's radius has been gradually diminishing and that the compensation therefor has been greatest in the oceanic basins, the areas of greatest rock densities.

It is now more than fifty years since James D. Dana began to teach that the rising continents and the sinking oceanic basins have been, in the main, permanent features of the earth's surface. He did not mean, however, that the continents have always had essentially the same shape, elevation, and areal extent that they have today. Still, Dana did not fully appreciate the amount of continental fragmenting that has taken place in the course of geologic time, though he clearly pointed out the foundering of Australasia, speaking of it in his famous *Manual of Geology* (p. 797) as "a fragment of the Triassic world." The teachings of Dana as to the permanency of continents and oceanic basins have been accepted in some form by all geologists, and lie at the basis of all zoögeography and evolution as well. Geologists are holding more and more to the hypothesis that the earth periodically shrinks, and each time it does so some parts or all of the continents may rise, but that in the main there is subsidence of the ocean bottoms, that the water of the hydrosphere is constantly increasing in amount, and that even though the continents are in the main permanent, yet they are partially breaking down into the oceanic basins.

From this we conclude that the enlarging oceanic basins are the most permanent features of the earth's surface. On the other hand, along with the progressive subsidence, the bottom of the Pacific is also built up into many local volcanic cones by outpourings of lava, and, further, it rises into more or less long mountain ridges. Some of these elevations of the bottom appear at the surface of the ocean as groups or lines of dead or active volcanoes. Another general conclusion is that most of

the "deeps" of the Pacific Ocean situated between 18,000 and 31,800 feet beneath the surface occur near the continents that exist now or existed formerly, or that they are located on the outer or oceanic side of mountain chains. These, the "foredeeps" of Suess, are striking tectonic features of the lithosphere. As for the true limits of the Pacific Ocean, Suess states that they are seen in the trends of long mountain folds. "So it is from New Zealand and New Caledonia to the borders of eastern Asia, to the Aleutians, and all along the western coast of both Americas."[1]

So far we have been considering the problem of crustal depressions essentially from the standpoint of hypothesis; now let us see what is actually known as to the topography of the Pacific Ocean and the geologic history of the Australasian region. An excellent summary of the present geography of the Pacific Ocean and the topography of its bottom is shown on the splendid map by Max Groll, recently published by the Institut für Meereskunde of the University of Berlin (1912). This map is based on Lambert's equal-area azimuthal projection, with a replotting of all geographic and bathymetric data ascertained up to January 1912, and is therefore more up-to-date and far better than any heretofore published. Groll states that he considered at least 15,000 soundings, made in all the oceans, and that yet there are many areas in the Pacific, hundreds of miles across, without a single one. It is therefore natural for him to add: "The greater part of the Pacific Ocean is still unexplored. . . . One is actually frightened at the little that is yet known of the bottom relief of the oceans and at the few data on which our representation of it is based. . . . Even in so relatively well known an area as the East Australasian seas, there are rarely more than from four to six deep-sea soundings to each five-degree field." Our detailed knowledge of the actual configuration of the bottom of the Pacific is therefore seen to be very slight indeed.

Let us now review the larger features resulting from the ancient cycles of aërial erosion and marine deposition through which has been determined the paleogeography of Australasia. An analysis of this history since the Cambrian seems to show that at least two northeasterly trending troughs of sedimentary accumulation began to form early in the Paleozoic. The western one, which may be known as the Tasman trough, almost wholly of Paleozoic development, is now partially elevated into the plains and mountains of eastern Australia, while the rest of it has sunk deep into the present sea, and with it considerable of what was formerly western New Zealand. The other, or eastern trough, which also appeared early in the Paleozoic, maintained itself after this

era in diminished extent throughout the Mesozoic and even into Pliocene time. This may be known as the New Zealand trough, a far narrower but much longer one than that of Australia; the shorter southern portion has now risen into the mountains of New Zealand, while the far longer northern part has apparently subsided to a depth of not more than 9000 feet, forming a submerged plateau upon which stand the volcanic islands of the Kermadecs and the Tongas.

In the New Zealand trough there appear to be, according to Park,[2] no less than 45,000 feet of Paleozoic and 11,000 feet of Mesozoic sediments, all of which are apparently of marine origin. These are coarse in grain and have much interbedded igneous material, which indicates that the adjacent lands were unstable and repeatedly reëlevated into high lands. There were at least four times when the New Zealand trough was markedly subject to folding and uplift; these were toward the close of the Silurian, Devonian, Jurassic, and Cretaceous periods. During the Tertiary, the New Zealand trough also appears to have been in continuous subsidence from late Eocene into Pliocene time, when about 9000 feet of marine sediments had been laid down along the eastern sinking margin. Late in the Pliocene there was marked vertical uplift, probably as much as 4500 and possibly even 6000 feet. The nearly horizontal Tertiary strata are now found in places at an elevation of 3000 feet, having been depressed 1500 feet during the time of Pleistocene glaciation. The high condition of New Zealand at this time united into a greater New Zealand all of the present outlying islands of the New Zealand plateau, no part of which is now submerged more than 3000 feet.

In Australia there is no evidence of the Tasman sea during Cambrian time, for the marine invasions at first are from the south and later across the entire medial portion of the continent. The trough begins to appear as a sea-way in the Ordovician (?5000 feet of deposits, according to Süssmilch[3]), with the greatest time of subsidence during the Devonian (27,000 feet); it continued with some interruptions throughout the Carboniferous and Permian (36,000 feet). During the Paleozoic, about 70,000 feet of essentially coarse sediments and interbedded volcanics were laid down in New South Wales, though smaller thicknesses seem to prevail elsewhere in eastern Australia. Here again we see the geologic results of high adjacent and often rejuvenated western lands. The record also shows that there were in Paleozoic time at least three periods of decided crustal folding (Ordovician, Silurian, and Devonian), and one of vertical uplift with faulting (during the close of Permian time). Following the Permian deformation, the continent was repeat-

edly lifted above the embrace of the Tasman sea, and most markedly so in the Pliocene, when all of eastern Australia was vertically elevated and block-faulted between 1500 and 7300 feet above sea-level (during the 'Kosciusko epoch'). In compensation for this elevation the Tasman sea sank, as now there are great depths close to the continent, in one place going down to 18,500 feet.

Australasia has been the most remarkable asylum among the continents for the preservation to this day of living examples of the plants and animals of the medieval world. Among these in great variety are the marsupial or pouch mammals, and the far less diversified, more primitive, but more remarkable egg-laying monotremes. The marsupials were at their culmination in the Pliocene, when forms existed larger than any living rhinoceros (*Diprotodon australis*). From the chronogenesis of these stocks and their diverse evolution in Australia, we learn that they must have been on that continent long before, and that they had been free from all Asiatic invasions and therefore escaped destruction by the higher, more intelligent carnivorous placental mammals. We must therefore conclude that Australia has been an island continent at least since late Eocene time, for it is since then that the placental mammals have elsewhere dominated all other land life.

The question next arises, When was Australia severed from Asia? From the paleogeography as now deciphered, we learn that Asia and Australasia were in complete connection throughout the early Paleozoic to the close of the Devonian. In Lower Carboniferous time, however, southeastern Asia began to be invaded by the Indian and Pacific oceans in the region of what are now the East Indian Islands. A greater subsidence here and in New Guinea, New Caledonia, and elsewhere in Australasia began in the Jurassic and probably persisted well into Cretaceous time. However, from the fact that carnivorous dinosaurs—land reptiles that arose either late in the Permian or shortly afterward—are known in the Triassic of Australia (none at all occur in New Zealand), we must conclude that there were still at this time intermittent land-bridges connecting this continent with Asia. The time of complete severance apparently came in the Jurassic, and the trough of separation seems to be the present Molucca-Banda sea, which has depths varying between 4650 and 21,100 feet.

Finally we must ask, When did the thousands of oceanic islands—the Oceanides—arise? They are probably in the main of volcanic origin and occur singly, in groups, and most abundantly in linear arrangement. The isolated and the grouped islands probably all represent great volcanic cones that have built themselves up from the ocean

bottom through the eruption of rock material. What is the origin, however, of those that are arranged in linear series? Are they ranges of volcanoes that have likewise grown from the depths but are situated on lines of fracture in the lithosphere, or do they rest on the crests of great arches or foldings of the ocean bottoms? Equally important questions are: What is their geological history, and have they simultaneous or successive origins? So far as known, none of the smaller oceanic islands reveals fossils older than the later Tertiary, a condition that appears to be in harmony with the theory that the sum of their movements is negative and thus in keeping with the idea that the oceanic bottoms are subsiding areas. We have as yet little to show when they originated, and still on the basis of the periodically recurring diastrophism it would seem that none can be older than the Permian, a time of intense and world-wide crustal deformation. Others may have originated during the late Cretaceous crustal movements, and all may have again been reëlevated and stirred into volcanic activity with the world-wide crustal readjustments that began in the Miocene and continued into late Pliocene time.

The views just presented are those of most paleontologists, but there are geologists and zoögeographers who do not accept the idea of continental fragmenting taking place on so large a scale as is here indicated. They hold firmly to the theory of the permanency of continents and ocean basins, believing that these positive and negative elements of the earth's surface have always retained the forms they now have. In their eyes, the physical evidence in the areas of fragmentation, and especially in the southern hemisphere, is not of a nature to compel the view that large lands formerly existed here, and they say, further, that there is no process in the mechanics of the earth known to them that would account for such down-breaking of the lithosphere.

As for the ancient life found in Australia, those who hold the above view say that we are still too ignorant of the world's organisms and their histories to conclude from them that their asylums were formerly connected with other land-masses, or they hold that the animals reached these places by accidental dispersal, through the air or by being rafted across the intervening water areas. Hence this conflict of views marks one of the greatest outstanding problems of geology and paleontology. The writer, however, is overwhelmed by the facts revealed in the geographic distribution of ancient and modern animals, and is compelled to dissent from the rigid view of the permanency of continents.

To sum up, in conclusion, we may say that the bottom of the Pacific Ocean in the region of greater Australasia seemingly became more and

more mobile with the Lower Carboniferous and especially during the Jurassic and Cretaceous. During this very long time, the eastern half of the continent, a land about 1800 miles east and west and 2200 miles north and south, nearly all went down more and more beneath the level of the sea to a maximum depth of about four miles and an average depth of between one and two and a half miles. Further, the entire area of the Oceanides also subsided, and possibly to an equally great depth; while this was taking place the bottom was apparently folded and built up by volcanic material into many more or less parallel ridges, a series of arcs extending over an area of about 3500 miles east and west and the same distance north and south. Finally, we may add that the entire western half of the Pacific bottom appears to be as mobile as any of the continents of the northern hemisphere, with the difference that the sum of the continental movements is upward, while that of the ocean bottoms is downward. This paper will be published at greater length and with illustrations in the *American Journal of Science.*

[1] Suess, *Natural Science*, 2, 180 (1893).
[2] Park, *Geology of New Zealand*, 1910.
[3] Süssmilch, *Geology of New South Wales*, 1911.

THE PETROLOGY OF SOME SOUTH PACIFIC ISLANDS AND ITS SIGNIFICANCE

By Joseph P. Iddings

BRINKLOW, MARYLAND

Read before the Academy, April 17, 1916. Received, May 12, 1916

Petrology as a more comprehensive term than petrography embraces all the phenomena and material characters of rocks, as well as the theories regarding their origin and the relations between the rocks of the earth and the problems of geodynamics. Knowledge of the composition, mode of occurrence, and distribution of igneous rocks should contribute materially to the elucidation of those problems in geology which are concerned with the constitution and behavior of the outer and inner portions of the earth.

For many years some geologists and petrologists have been convinced that the various kinds of igneous rocks in different parts of the world, both volcanic outflows and intruded bodies, are so intimately related to one another within each region that they must have been derived from some parent lava, or rock magma, by processes of physico-chemical differentiation; and further, that in different regions of the earth the series of igneous rocks in each possess chemical and mineral

characteristics which distinguish them from series of rocks in other regions. In short, that the series of igneous rocks in various parts of the world are not exactly alike, though some may be.

Recently I have called attention to the apparent relation between regional characteristics of igneous rocks and the broad demands of the theory of isostasy. The igneous rocks of the North American continent, both as a whole and when considered in regional groups, such as those of the Pacific cordilleras, the Colorado plateau, or the Atlantic coast, average lighter than the igneous rocks of islands in the deep oceans such as Hawaii in the Pacific, and Réunion in the Indian Ocean. The relative densities of a completely crystallized average magma of the Pacific Coast rocks, and those of average magmas for Hawaii and Réunion are to one another as 2.85 to 3.12, for each of the island groups of rocks. This appears to be in accord with the demands of isostasy, if igneous rocks represent the material forming deep portions of the lithosphere, or material immediately below it, or at its base. However, much remains to be done in studying the igneous rocks of all parts of the earth, especially of the little known islands of the Pacific Ocean.

It was with the hope of contributing something to the solution of the problem of isostasy from the petrographic side that I undertook recently a reconnaissance of some volcanic islands in the South Pacific, namely: Tahiti, Moorea, the Leeward Islands: Huahine, Raiatea, Tahaa and Bora Bora; besides the two largest islands of the Marquesas, Hiva-oa and Nukahiva.

So long ago as 1849 James D. Dana published an excellent description of Tahiti, and mentioned the occurrence of a variety of syenite among the basalts of this volcano, which he considered to be "only a feldspathic variety of the same igneous rocks that constitute the island;" a perfectly correct assumption. Darwin visited Tahiti a few years earlier than Dana and noted the basaltic character of the rocks. In a conversation with the writer 30 years ago Dana remarked "If you want to study a dissected volcano, go to Tahiti." At that time it seemed a difficult thing to do. Today the island is on a direct mail route between San Francisco and Wellington, N. Z.

In 1910 Lacroix published a description of Tahitian rocks which were in the Museum of National History in Paris. Besides abundant basalts the collection embraced coarsely crystallized gabbros, syenites and lavas that are characterized by nephelite and haüynite, minerals generally considered absent from igneous rocks occurring in the region of the Pacific Ocean, but already found to some extent in the Hawaiian and Samoan Islands.

Professor Marshall, of Dunedin, N. Z., has described nephelite-bearing rocks from the Cook Islands and from some of the Leeward Islands in the Society group; and in 1915 he published an account of his journey to the center of the island of Tahiti. From these descriptions it was known that all of these islands are basaltic and that some of them contain trachytes and phonolites, but the relative abundance of these rocks and the more specific characters of the basaltic lavas were not definitely known, and no approach to an estimate of the average magmas of the several islands could be gained from the fragmentary nature of the information at hand.

A reconnaisance of the islands was thought to be worth the effort, and has been productive of valuable results. However, a great deal remains to be done in the way of more accurate detail exploration of the islands than I was able to carry on in the few months at my disposal, and with the limited means at my command.

Tahiti, the largest island in this region, is an extinct volcano, deeply eroded by streams that have cut canyon-like valleys, which radiate from a circular range of high mountains, surrounding the deep central basin heading Papenoo valley, which drains northward. This central basin is 5 or 6 miles across from crests of the encircling range with its peaks of 4000, 5000, 6000, and in Orofena of 7000 feet in altitude.

In the geometrical center of the island, which is in the western head of the Papenoo Valley, there is a low conical hill, Ahititera, composed of coarsely crystallized rocks, gabbros, peridotites with subordinate amounts of nephelite-syenite and other rocks. The surrounding rocks, so far as seen through the forest of tropical vegetation, are basaltic tuff breccias and basaltic lava flows, the latter preponderating in the upper parts of the mountains and in the outlying spurs and slopes of the ancient volcano.

The top of the central hill, Ahititera, is about 2500 feet above sea level, and may be assumed to be about 7000 feet below what was at one time the bottom of the final crater of the volcano, if the mountain had the same profile as the great modern volcanoes of Hawaii. The valley bottoms are nearly level for miles inland from the coast, and their heads lie deep below the summits of the mountain ridges between them. Their sides are very steep, in many places almost vertical walls, thinly covered with clinging ferns and vines.

The lavas composing the Tahitian volcano are basalts rich in ferromagnesian minerals, which commonly appear as prominent crystals. Feldspars are seldom seen, but are prominent in some varieties of the basaltic lavas. There are very few lavas with a trachytic appearance

on the main island of Tahiti, though they are common on the lesser volcano of Taiarapu which is connected with Tahiti by a narrow isthmus.

The preponderating lavas of the Taiarapu volcano are basalts like those of Tahiti proper. The smaller volcano has been deeply eroded, and a central valley reveals a core of coarse-grained rocks: gabbros and peridotites, which represent the lavas that solidified in the conduit of this volcano when it became extinct. No syenites were found among the rocks, although trachytes and phonolites are common among the latest lavas in the upper, or marginal, parts of this volcano. Such alkalic feldspathic lavas are clearly differentiates of the basaltic magma, the complimentary varieties being peridotites and peridotitic lavas which are characteristic of these and neighboring volcanoes.

Of the chemical analyses of Tahitian rocks already published by Lacroix two are of preponderating varieties of basalt, and two are of preponderating varieties of gabbro from the central core in Papenoo Valley. The four analyses agree closely with one another and their average may be considered a first approximation to what was probably the parent magma from which all the lavas of Tahiti were derived. The estimated specific gravity of a completely crystallized rock of such a composition is 3.18. The average of the relative densitites of 4 specimens of gabbros and peridotites from the Papenoo Valley is 3.18, and of 4 similar rocks from the central valley of Taiarapu is 3.16. These estimates leave out of account the syenites, trachytes and phonolites of the island, for the amount of these rocks when compared with the bulk of the basaltic volcanoes is almost negligible.

This preliminary estimate of the relative density of the solidified magmas of these two volcanoes may be slightly too high, and is somewhat greater than those calculated for the solidified magmas of Hawaii and Réunion. It clearly indicates that the magma from which the Tahitian volcanoes sprang is sufficiently heavy to correspond to the great depth of the Pacific Ocean in this region according to the theory of isostasy.

Moorea, or Eimeo, is an island volcano ten miles northwest of Tahiti, which has been greatly eroded and partly submerged under the sea. The central portion has been reduced to a deep valley surrounded by high precipitous mountains 2500 to 3500 feet in height. The upper portions are composed of horizontal layers of basalt lava. The central basin is drained northward by two valleys, drowned by the sea, forming bays with a high mountain ridge between. Small, radial, valleys cut the main ridge into short spurs which pitch steeply to the coast. The exposed lava sheets are seen to dip away from the center of the

island toward the ocean, and show that the volcano originally was a flat-topped dome, the upper portion of which was mostly solid flows of lava, while the lower middle part of the volcano was largely tuffs and breccia.

No core, or conduit, of coarsely crystallized rocks has been exposed by erosion, which is an indication that the volcano of Moorea was not as high above the sea as those of Tahiti and Taiarapu. The deep bays in Moorea show that this volcano has been flooded by the sea to a considerable depth, while the absence of bays on Tahiti indicates that this island and Taiarapu have not sunken sufficiently to submerge the deeply eroded valleys, although the lower portions of the stream channels are only a few feet above the surface of the sea. The principal lavas of Moorea are basalts like those of Tahiti, but there are subordinate though large bodies of trachytic and phonolitic lavas, and small bodies of peridotitic rocks.

The Leeward Islands, or the Society Group, 100 miles northwest of Tahiti, are similar in structure and in the character of their lavas. Huahine, the most easterly, is a volcano so greatly eroded and flooded that a narrow, shallow, strait separates the northern from the southern portion, and connects the heads of east and west bays. The eroded center of the volcano is submerged in the head of the east bay. The mountains and ridges, with spurs sloping seaward, consist of lava flows nearly horizontal in the central peaks, but dipping down the spurs and ridges in all directions toward the coast. The basaltic lavas have the same general composition as those of Tahiti, but there are mountain masses of phonolite and trachyte in several parts of the island.

Raiatea, the largest island of this group, consists of basaltic breccias at its center, with basaltic lavas in sheets which are horizontal in the upper parts of the central mountains, but slope toward the coast in the spurs. A great sheet of trachytic lava caps the long ridge forming the middle of the northern portion of the island, and reaches the shore at the ends of the northwest spurs. There are also several mountain masses of phonolite. The volcanic center of the island has been eroded to a deep valley draining east and flooded by the sea. There are several other drowned valleys on Raiatea.

Tahaa, which is within the same lagoon and barrier reef as those surrounding Raiatea, is another dissected volcano having a deeply indented coast with bays extending to some distance inland, and having lava flows dipping down the spurs to the sea, and more or less horizontal in the central portion.

Farther west, the smaller island, Bora Bora, is much more reduced

by erosion, and consists of a central mountain about 2500 feet in height, with a vertical escarpment of fully a thousand feet which exposes horizontal basaltic lavas. The marginal ridges surrounding two deep bays are the remnants of volcanic slopes in which the lava sheets dip outward toward the surrounding lagoon.

The still smaller island of Maupiti is the remains of a basaltic volcano almost completely submerged in the ocean. The encircling lagoon is relatively larger than those about the less submerged islands farther east. In fact there is a progressive relation between the width and depth of the lagoons around these islands and the apparent submergence of the volcanoes. The most deeply submerged islands and relatively largest lagoons are in the west or northwest, that is, in the Leeward islands, and in the northwestern part of these.

There is less submergence and the lagoons are less well developed at Moorea and Tahiti. However, in this, the Georgian group, Tetiaroa, 26 miles north of Tahiti, is only an atoll, while Mehetia, 60 miles east of Taiarapu is said to be an extinct crater 435 meters high, having a peak to the north and a gentle slope to the south, and having a difficult coast.

The islands of the Marquesan group, 600 miles northeast of Tahiti, are extinct and eroded volcanoes consisting of heavy basalts similar to those of Tahiti and the Leeward Islands, with quite subordinate amounts of trachytic lavas. Erosion has cut deeply into the volcanic mountains in places and has carved steeply walled valleys, surrounded by precipitous mountain ridges, with some commanding peaks, as at Traitors Bay and the Bay of Hanaiapa on Hiva-oa, and along the south coast of Nukahiva. This coast is very rugged with cliffs 1000 feet high, topped by hanging valleys.

The coasts of Nukahiva and of Hiva-oa plunge steeply into the sea which in places is a thousand feet deep within a few hundred feet from shore. No coral reefs surround these islands, though a few fragments of corals are found on the beaches which occur at intervals along the coast. Small reefs of coral are said to occur in some localities in this region. The northwestern side of Nukahiva is a long, gentle volcanic slope reaching sea level.

In the Marquesan Islands, so far as my observations go, sea erosion appears to have progressed more rapidly than stream erosion in places where sea cliffs rise to great heights, with hanging valleys and with waterfalls plunging into the sea. However, there are great differences in the topographic relief in neighboring parts of the same island, as on Hiva-oa, where in the western portion deep valleys have been cut with

mountainous walls and sharply serrated dividing ridges, while in the middle portion of the island there are broad expanses of rolling highlands which are drained by shallow channels, or short steep gulches.

The high massive cliffs along parts of the coasts and around some of the largest valleys are probably due to the fact that the upper thousand feet of the ancient volcanoes consisted of superimposed sheets of dense lava, beneath which are breccias and tuffs which yield much more readily to erosion and permit the overlying massive portions to be undermined. From the great depth of the sea off the coast and within the bays, as well as from the absence of coral reefs or of extensive sea shelves, it appears probable that these islands have been submerged to a considerable depth within comparatively recent times.

IN RELATION TO THE EXTENT OF KNOWLEDGE CONCERNING THE OCEANOGRAPHY OF THE PACIFIC

By G. W. Littlehales

U. S. HYDROGRAPHIC OFFICE, WASHINGTON, D. C.

Read before the Academy, April 17, 1916. Received, May 24, 1916.

The body of water whose oceanography is under discussion is of an extent so vast that its area exceeds by 10,000,000 square miles the total land surface of the globe, and its cubical content is estimated to be seven-fold greater than all the land above sea-level. The indications are that throughout nine-tenths of its expanse the depths are greater than one mile, and throughout three-fourths of its expanse the depths are greater than two miles. It is the field of the interplay of many different forces exercising an important influence in terrestrial physics, and presents a realm of unsurpassed promise for the fruits of investigation.

The accumulated oceanographical observations in the Pacific relate principally to the surface and the bottom. Even these are deficient, and the intermediate depths have been much less investigated. The materials from centuries of voyaging and from the expeditions for sounding the ocean sent forth since the last quarter of the nineteenth century, when deep-sea soundings first began to be taken in the Pacific, have provided information of the general distribution of barometric pressure and winds over this vast tract and also of the general aspects of surface circulation, temperature, and salinity; but the details of these matters have scarcely been touched. Until the tides have been gauged in the open ocean away from the land, it is not likely that a clear solution of the tidal problem will be completed. Despite the

tidal observations hitherto gathered on the islands of the Pacific and on the coasts of the countries bordering this ocean, the means for the construction of the cotidal lines are either wanting or deficient in many parts.

The manuscript sheets of the Bathymetrical Chart prepared by the Hydrographic Office of the Navy Department, containing all the authentic deep-sea soundings, show the small extent to which the basin has been sounded and the distribution of bottom deposits made known, and prove the inadequacy of existing measurements to define the general contours of configuration. Even the continental shoulder is imperfectly known around most of the ocean circuit. In the North Pacific there is a tract twice as large as the United States which has been crossed by only a single line of bottom soundings about 250 miles apart; and in different parts of the Pacific other tracts as large as the continent of Australia remain entirely unfathomed. The majority of soundings are grouped in two lanes running respectively from the United States to Australia and from the Hawaiian Islands to the Philippines and Japan. No trustworthy contours of equal depth can be drawn beyond the continental shelf, because of insufficient information. Where knowledge is so imperfect, surprises should be in store for those who have accepted the representations appearing in depth-maps of the Pacific in which the isobathic contours have been courageously supplied. The soundings have generally been spaced too far apart to admit of conclusions in relation to even the grosser aspects of the orography, and hence the shape of nearly one-half of the earth is little known.

The deposits on the floor of the ocean have been reached at relatively few points and have generally been penetrated only to the depth of a few inches. The dearth of knowledge of their thickness and stratification makes it desirable to devise methods of bringing up a core several feet long. Although it is extremely doubtful whether these deposits have any analogues in the terrestrial rocks, the interest in the evidences of their stratification arises on other grounds. For instance, if a layer of red clay in great depths should be found overlying a layer of globigerina ooze, the induction would be that the floor of the ocean at this station had once occupied a position at a less depth below the surface of the ocean than the one in which the red clay was deposited; and this would bear strongly on great problems of crustal deformation.

The few records of serial temperatures and of the analyses of water taken from various depths, show that the Pacific bears a close resemblance to the other oceans in the general distribution of temperature,

salinity, and density. That is to say, the waters have a gradual increase in density from the surface to the bottom, a gradual decrease in salinity from the surface to the bottom, and a gradual decrease in temperature from the surface to the bottom. But the details of the distribution of these quantities is unknown; of their variations from season to season and from year to year few observations have been made in the depths of the Pacific, and there is as yet very limited knowledge of the import of such changes upon the variations of climate and of physical and biological oceanography.

The observational foundation for investigating the ocean from the standpoint of thermodynamics requires the study in detail of definite stations occupied in concert and periodically revisited for the purpose of observing, as nearly as possible at the same time, such physical conditions, at certain depths, as the temperature, the salinity, the gas-content, and the currents; and in this way affording the means of presenting in the form of synoptic charts the changing network of lines of equal values of the physical elements in their distribution in the depths.

To sum up our thoughts, we may fix attention (1) upon the basin, of which no model can be at present constructed; (2) upon the deposits, whose thickness and stratification still remain unrevealed; (3) upon the waters, whose variations in physical conditions have not yet been sufficiently observed to explain the inner mechanism by which they operate to produce their effects in the economy of the earth.

MARINE METEOROLOGY AND THE GENERAL CIRCULATION OF THE ATMOSPHERE

By Charles F. Marvin

U. S. WEATHER BUREAU, WASHINGTON, D. C.

Read before the Academy, April 17, 1916. Received June 13, 1916

Stations for the surface observation of meteorological conditions are now numerous in the principal civilized countries of the world, and within the past twenty-five or thirty years explorations and investigations of the free upper air have been actively conducted at a number of places on land. In a few instances soundings of the air have been made on the North Atlantic ocean and in waters adjacent to Europe under the initiative of the late A. Lawrence Rotch, Teisserenc de Bort, and others. As a whole, however, little is known in detail of the meteorology of the oceans, except as revealed by the simple observations of weather and wind which many merchant and naval vessels have been accustomed to report for a number of years while plying their regular

courses. Observers at sea on such occasions have but scant opportunity or incentive to engage in serious scientific observations, and difficult investigations are impossible. Consequently, what has been obtained in the past through these opportunities, and what may possibly be thus obtained in the future, is limited and restricted in every way, that is, limited as regards the kind and quantity of data that may be obtained, and restricted as to the region or locality to which it applies.

A proposal to launch and equip an expedition to make a scientific exploration of the Pacific Ocean is, therefore, hailed by the meteorologists with enthusiasm.

The aerological investigations now being made at a very few continental stations by means of kites and balloons constitute but a fragment of the evidence and data needed to exhibit the more detailed features of the circulation of the atmosphere. Data from and over the ocean can be obtained only by means of *vessels devoted exclusively* to scientific investigations, and the vast stretches of the Pacific, dotted with its occasional islands that afford useful vantage points as bases of reference, offer a field for such explorations unsurpassed elsewhere.

To state the proposal and indicate the objects of the exploration seem to be all that is necessary to enlist the fullest support thereof. It offers to meteorology the only opportunity possible to obtain full and complete observational data prepared by experienced and competent observers qualified to conduct the difficult exploration of the free air now so much needed. With the few exceptions previously alluded to, upper air explorations have been made only at a small number of continental stations, located at a few points in England, France, Italy, Belgium and Germany. These, in a sense, constitute only a small group in the aggregate and are supplemented by a few additional detached points of observations in the United States.

Both the surface and the free air observations from the ocean are valuable in themselves as supplementing the corresponding continental data, but the opportunity presented by a special expedition moving from point to point in both latitude and longitude on the greatest of all oceans permits of extending observations by the same standard methods to those regions from which the information is most valuable and most needed.

Progress in the development of our knowledge of the upper air and its general circulation awaits accurate observations by the aid of kites and balloons. The scanty observations from a few stations on limited and scattered continental areas show that the atmosphere arranges itself in two or more well-defined layers of different characteristics and

extent. Little is known and much is as yet conjectured as to the real boundaries, dimensions, and characteristics of these several strata, even over land areas. Only an expedition like that proposed can suffice to extend such studies to our greatest ocean and over which there is every reason to suppose the atmosphere disposes itself in its best defined, simplest and most orderly arrangement because an ocean represents an almost unlimited extent of level and uniform surface conditions, accompanied by stable and uniform gradients of temperature and other meteorological conditions. The modifying effects experienced at land stations due to their elevation above the sea, their local topographic environment, and other disturbing causes are wholly absent or inappreciable over the ocean, whence marine observation may be expected to supply, not only a kind of data the meteorologist greatly needs, but the best data of that particular kind.

By the aid of suitable recording instruments carried aloft by balloons we may obtain, if the instruments are recovered, a record of the temperature, pressure, and moisture of the air, the sunshine, and possibly some other conditions. However, as a free balloon simply drifts along with the air strata through which it rises and falls, no record of the *motion* of these strata can be procured from the balloon itself. To ascertain the *motions* we must continuously triangulate the successive positions of the balloons. This calls for two or more observers, with appropriate theodolites, located at the ends of a suitable base line. Such observations are often not even attempted as a part of aerological work, but they are indispensable in studies of the circulation of the upper air. No other observations are more urgently needed in meteorology at the present time than these, and it is difficult to conceive of a better field for conducting such observations on a broad and all-inclusive plan and scale than the Pacific Ocean. These motions of the free air, in conjunction with the pressure and temperature thereof, are the data most needed to verify or disprove, or rather, if possible, to adapt and apply Ferrel's general theories to the motions of the air as they may be actually observed on an expedition of the kind proposed.

Emphasis has been laid upon the aerological work which a Pacific exploration could perform, because the need for this is greater, perhaps, than for any other phase of meteorological investigation, and also because the expedition provides such exceptionally favorable opportunities for its prosecution. Nevertheless, the complete study of surface conditions, the observation of clouds, fogs, waterspouts, auroras and lightning, including the photographing of such phenomena, and the intensive study of the inception, development,

progress and decline of typhoons and similar marked storms constitute a program of work that claims the careful attention of those who plan the expedition, and can not fail to command the interest and approval of men of science generally.

ON THE DISTRIBUTION OF PACIFIC INVERTEBRATES

By Wm. H. Dall

SMITHSONIAN INSTITUTION, WASHINGTON, D. C.

Read before the Academy, April 17, 1916. Received, May 24, 1916

The distribution of Marine invertebrates is important, as one of the keys to the former distribution of land masses, and to our very imperfect knowledge of their distribution in the Pacific. Certain species, usually those inhabiting the reefs and comparatively shallow water, are very widely distributed over the region usually referred to as Indo-Pacific; but when a careful collection of the species belonging to any isolated island or group is available it becomes evident that a large proportion of them are local and combine to form a local fauna. A knowledge of these faunas is necessary before any satisfactory discussion can be had of the presumably Tertiary fossiliferous deposits which are found fringing the more elevated Pacific islands. The landshells of the Hawaiian and Tahitian groups indicate a high antiquity for their isolation according to Pilsbry, the most eminent student of these animals. The facies of the Tertiary fossils obtained by Ochsner on the Galapagos Islands indicates a derivation from the American rather than the Indo-Pacific fauna, with which the recent invertebrates are commingled. These facts indicate the interest which attaches to a wider knowledge of the Pacific faunas.

THE MARINE ALGAE OF THE PACIFIC

By W. G. Farlow

DEPARTMENT OF BOTANY, HARVARD UNIVERSITY

Read before the Academy, April 17, 1916. Received, May 24, 1916

In considering the desirability of an exploration of the Pacific Islands the following points relating to our knowledge of the marine algae of the Pacific may be mentioned. Our present knowledge is so fragmentary that it is not possible as yet to suggest any special problem of a general nature whose solution would be aided by a well arranged expedition. What the important general questions are we cannot tell until after a more thorough exploration has given us a more detailed knowledge of

the species which inhabit the islands of different groups. It may be well therefore to give a general review of what we know from previous explorations of the different portions of the Pacific and at the same time to distinguish between those regions whose marine flora is now being studied successfully either by native algologists or by already organized bodies of collectors and explorers and those regions which are still in need of exploration by some specially organized expedition.

In this connection what is said must be understood to apply to the alga flora proper which is littoral and does not extend to any great depth and of which very few or no representatives are to be expected in deep soundings. In regard to the pelagic or plankton flora it may be assumed that no expedition would fail to make collections wherever and whenever possible.

Our knowledge of the algae of the Pacific has been obtained in part from the collections of the earlier national expeditions, few of which were accompanied by expert algologists, or from collections of a more recent date made by a very small number of residents of different islands who were interested in algae and by botanical travellers stopping for brief periods on their journeys across the Pacific. Islands lying on the trades routes have naturally been more frequently explored than others. In the earlier expeditions, even those whose object was ostensibly the exploration of the Pacific, we notice that the greater part of the time was spent in exploring the outlying islands as Hawaii, Japan, and New Zealand. In the Wilkes Expedition, which may be taken as a type of the old national expeditions, the algae collected were largely from Brazil, the West Coast of America, New South Wales and New Zealand, while hardly fifty species were obtained at Hawaii, the Fiji Islands and Tongatabu, with half a dozen, not including Diatoms, from the Philippines. In the majority of expeditions Hawaii, the Fiji Islands, and the Friendly Islands, were visited. Our knowledge of the Eastern Polynesian groups, as the Society Islands, the Marquesas Islands, and the Low Archipelago is derived largely from the early French expeditions, as that of the Bonite, that of D'Urville, Au Pol Sud, and that of Jardin to the Marquesas. Tahiti, however, was visited by numerous expeditions and it has been a favorite resort of botanical travellers in recent years. On the other hand, of such remote, small islands, as Pitcairn's and the Gambier Islands we have only the scantiest information. We are not to assume, however, that because very little has been found on the smaller islands that there is not much more which may be found.

The Hawaiian Islands have been visited by more expeditions than any other of the Pacific Islands, and their marine flora has lately

been studied by competent algologists who have made large collections during a residence there; hence a new expedition will not spend more than a comparatively short time there. The case of the Philippines is peculiar. Although many expeditions have stopped there, the collections have been scanty and probably they do not fairly represent the marine flora of those large islands. Nevertheless it does not seem to be advisable to send a special expedition there for the study of algae because there is now in the Philippines a number of resident botanists who are able to collect year in and year out; and more is to be expected of them than from an expedition moving from one part of the Pacific to another. On the other hand the relation of the flora of the Philippines to that of the Caroline and Ladrone Islands ought to be closely studied. We have a limited knowledge of the species of those two groups obtained from various expeditions; and quite recently the Japanese algologist, Okamura, has published a paper on the Caroline algae. Other regions the relation of whose marine flora should be compared with that of the Pacific Islands are the Bismarck Archipelago and adjoining regions of which there are numerous, short scattered notices and the more eastern islands of the Dutch East Indies where collections have been recently made by the Siboga Expedition. It is hardly necessary to send a new expedition to those regions whose flora is being well studied by competent experts.

After this statement of the regions to which it is inadvisable to send an expedition for the reasons given above, we may turn to a more exact consideration of the regions more in need of exploration. As has been said, the islands lying in or near the trade routes from Australia and New Zealand to North America have not infrequently been visited by botanists. These islands are the Fiji, the Samoa and the Friendly Islands groups. One of the more important papers on these groups is that of Grunow on the algae collected by Von Graeffe, including about 120 species. The distinguished algologist, W. H. Harvey, also issued a set of Friendly Island algae. From these and other papers we know the main features of these islands, but there must be much more to be learned about them. We would advise a new exploring expedition to make either Fiji or Samoa a base of operations. Starting thence as a centre, a year could be well spent in a careful study of the whole group of Polynesian Islands, the Friendly Islands to the Marquesas on the northeast and Pitcairn's Island on the southeast. Another year, or better more, should be spent in traversing the Solomon Islands to the Caroline and Ladrone groups returning by the Gilbert and Marshall Islands to Samoa and thence to Hawaii on the return voyage.

As will be seen, I have assumed that the exploration should last several years. A hurried trip with short stops at a few remote points is not enough at the present day. What we wish is a more detailed knowledge so that the fragmentary facts which we now possess may be fused into a more complete picture of the Pacific Marine Flora. Then and not until then shall we be able to discuss intelligently the question of the distribution and general character of the insular floras and their relation to those of the nearest continents.

The exploration of the marine flora is of course only one of the numerous objects of an expedition to the Pacific, but it is an important object when one considers the great extent of the coast to be studied. The work in this important field should be intrusted to an expert well trained in the study of algae and not left to the chance work of a general collector who, although he might bring back valuable material, could not be expected to recognize the smaller species which are quite as interesting, often more interesting than the larger species.

THE PACIFIC AS A FIELD FOR ETHNOLOGICAL AND ARCHAEOLOGICAL INVESTIGATION

By J. Walter Fewkes

BUREAU OF AMERICAN ETHNOLOGY, WASHINGTON, D.C.

Read before the Academy, April 17, 1916. Received, May 24, 1916

In the following suggestions regarding the Pacific as a field for anthropological investigation the valuable work already accomplished in this branch of study has not been overlooked. The contributions which, from time to time, have been published by local students have been most important, and the admirable reports of expeditions sent out by the different governments of Europe and by local museums, have revealed a wealth of material on the aboriginal inhabitants of the Pacific.

But the field for anthropological investigation in the Pacific is so vast that many additional laborers are needed to gather the plenteous harvest it offers. There is an urgent call for coöperation of many more specialists so that this work may be carried on as part of a coördinated plan, systematically followed. This necessitates more observations on the migrations of the Polynesians, Melanesians, and dark races of the Pacific, to determine their center of origin and diffusion from island to island, a study directly connected with geographical phenomena. Investigations of the colonization of the Pacific islands, of the migration of man from island to island, and of the effect of insular environment on human culture, are very attractive fields of research. Much remains

to be investigated in this line of study. Many direct observations are desirable to advance our knowledge of the Melanesians, Papuans, and especially those Polynesians, if any, that are still uncontaminated by civilization and missionaries, with a view to discover ways of primitive thought. We need a comprehensive study of the linguistics of the Pacific islands by linguists trained in modern intensive methods, in order to determine the relationship of the Melanesians and Polynesians. We have no collections of texts, few myths and only a fragmentary knowledge of the material culture of these peoples.

One of the most important lines of work among Pacific islanders is to gather from natives data of this kind which is rapidly being modified and will be lost forever if not gathered in the immediate future. Much remains to be done on the sociology of the Polynesians and Melanesians, notwithstanding the important observations that have already been made. There is an urgent call to collect material on terms of family relationship as a contribution to primitive society. The observations already made should be verified and augmented by new facts regarding the sociology of these fast disappearing natives.

There are many islands which offer facilities for determining the antiquity of man on the Pacific islands. Throughout Polynesia are scattered many prehistoric ruins of which comparatively little is known. Megalithic monuments occur not only on Easter Island, but also on Tahiti, the Marquesas, Tonga, the Carolines and Ladrones. Although Pitcairn island was uninhabited when discovered, huge carved pillars, massive walls and images, indicate the former existence of a characteristic culture. The great stone temple at Atahura in Tahiti, said to be 270 feet long, by 94 feet wide and 50 feet high, was approached by a flight of steps hewn out of coral and basalt rock, showing artistic skill beyond that of the present inhabitants. There are ancient stone platforms in the Marquesas constructed by people of mixed Polynesian origin which rank among the largest in Oceania. On Ponape of the Carolines carved stone images over eight feet high are reported; and at sacred Tonga there is a trilithon monument, consisting of two massive upright stone blocks into which is morticed a sculptured transverse megalith on which is a circular stone basin. Our knowledge of these monuments is very limited. Field work on them is very desirable.

Mr. Christian has published an entertaining account of the so-called Pacific Venice, an architectural marvel, situated in the Caroline Islands. This monument consists of ancient platforms and massive walls made of hewn basalt. It measures 185 feet in length, and from 20 to 40 feet in height. Within its enclosure on a raised inner terrace is a stone

mortuary chamber in which ancient chiefs of the island are said to have been buried. The work thus far done by archaeologists on these great monuments is comparatively superficial. Further investigations promise much additional material.

The islands of the Pacific present most instructive problems in physical anthropology. There is every evidence that renewed study in this line would reveal much new material.

The strongest appeal that the anthropologist can make for additional field work on the prehistoric inhabitants of the Pacific and their culture is their bearing on the unity or plurality of origin of man. Culturally the aborigines of America and those of the Pacific islands were in the Stone age when discovered. We naturally look to the Pacific for the cultural kin of the American race. It is desirable that extended observations be made on the Polynesians to supplement what is known of the Stone age of the continents, especially America.

I would naturally lay great stress on a systematic survey of the aboriginal monuments in the Pacific islands to discover their history. The first step would be to make an archaeological reconnaissance to determine the distribution and character of antiquities. Having determined in this way which one of the many sites of human occupation shows superficial evidences of the greatest age, excavation should be made upon it to ascertain its age, history and relation of former inhabitants, as shown by skeletal material, remains of architecture, minor artifacts and other archaeological material.

MID-PACIFIC LAND SNAIL FAUNAS

By H. A. Pilsbry

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

Read before the National Academy, April 17, 1916. Received, June 7, 1916

It may seem presumptuous to infer from the distribution of little land snails on scattered Pacific islands, that large Pacific land areas have disappeared in Tertiary time; but if we accept the general principle that similar organisms have evolved from a common ancestry, this bold inference seems justified. Briefly, the reasons are as follows:

The land snails of the Pacific islands, as far as now known, may be divided into two groups: those living on low islands and on the shore zone of high islands, and those inhabiting the forests of high islands. Many of the first group have a wide distribution suggestive of dispersal by human or other adventitious agency and are therefore not significant in the present problem.

The most striking features of the second group in Polynesia and Hawaii are: (*a*) The absence of all the highly evolved continental groups of land snails. There are no Helicidae, no slugs, no carnivorous snails, none of the operculate Cyclophorid snails—in a word, none of the groups that have constituted the bulk of all known continental faunas since the Eocene. (*b*) The prevalence of groups that belong, by their relatively primitive anatomical structure, to early branches of the phyletic tree of the land snails. Some, such as the Amber snails (Succinea), and the Endodonts, have relatives in Mesozoic continental deposits, and still form a small part of the modern continental faunas. Some, like the Helicinidae, are presumed from their structure to be ancient groups, early terrestrial adaptations derived from the most primitive group of marine Prosobranch snails. (*c*) Another series of families, the Achatinellidae, Amastridae, Partulidae, Tornatellinidae and Pupillidae, belong to a primitive group having pallial organs like the aquatic snails Lymnaea and Auricula. They form a primary division of the land snails called Orthurethra. One may compare their relation to the dominant groups of continental snails to the relation Monotremes bear to the placental mammals.

This low group is represented on the continents by such minute forms as the Pupillidae and Valloniidae, which were present in essentially their modern forms in the Eocene, and still seem to exist by grace of their insignificance, occupying a station which no higher group has been modified to fill.

In the island world, this primitive group has had a wonderful evolution. It comprises all of the large snails, and outnumbers all other groups. Shell forms have been evolved paralleling most of those of continental snails, except that up to this time no shell-less, sluglike form has been found. There are adaptations to all varieties of terrestrial and arboreal stations.[1]

The greatest variety of Orthurethra is in the mid Pacific; westward a few forms extend into islands also inhabited by some distinctively continental families of snails. Fiji is the extreme outpost of the latter. There they stop abruptly. The presence of this border of continental snails in the southwest Pacific has been looked upon by Hedley and others as evidence of former continental extension, while some zoogeographers attribute it to over-sea drift. The suggestion may be entertained that here a later fauna has been superposed upon an earlier.

Where shall we look for the source of this unique Pacific fauna? It is known that on the continents the chief existing families of land snails were already established in the Eocene; and that many of the

existing genera were represented in the Oligocene. If Pacific faunas were derived as has often been claimed, from waifs drifted thither on 'natural rafts' or carried by birds from the continents, such migration must have stopped as an effective factor in colonization before Tertiary times, for otherwise it seems unaccountable that *all* dominant Tertiary snails of the continents are absent, as with our present knowledge, they appear to be. There is small reason for believing that supposed means of mollusk transportation which have failed during the three or four million years of Cenozoic time, were any more efficient before that. It appears that here, as on the continents, their dispersal has been chiefly by their own known powers of locomotion.

It is difficult to escape the conviction that the archipelagoes of today are superposed upon folds of vastly greater extension, now lost by subsidence, upon which the Pacific snails were evolved, and which served as paths for their migrations. It may be inferred that in the Mesozoic or earlier, continental connections, or close proximity, gave the snails of that time access to the Pacific lands. It is not necessarily inferred that at any one time an area of continental proportions existed. It is of course heterodox to doubt the absolute permanence of oceanic basins, but I must confess that my faith in the good old doctrine has been shaken. It seems conceivable that the doctrine of isostatic balance is not incompatable with the idea that some former areas of low gravity, perhaps intermediate between that of the continents and the sea floors, have been depressed in late geological times.

The limits of my space do not allow discussion of much evidence favoring the distribution of mid-Pacific land snails by former land extensions. The great amount of differentiation among the primitive Pacific snails indicates a very long period of evolution—far longer than any estimate of the duration of the present volcanic islands. The affinities of island faunas are highly suggestive, even though so imperfectly known, of distribution along certain trends. The trade winds have always blown from eastward, yet there is absolutely no snail of a distinctively American group known in the Pacific faunas.

Some light may be thrown upon the subject of inter-island distribution or migration by a brief view at close range of a limited area. In the Hawaiian Islands, where alone I have had personal experience, no large species of land snail is found living on more than one island, but adjacent islands, such as Molokai, Maui and Lanai, have closely related species. The tree snails and allied ground snails inhabit mountain forests, mainly above 1000 feet, but before the destruction of the lower forests they came down to less than half that elevation, though not into

the shore zone. Each species has its definite territory, sometimes one, sometimes several adjacent ridges. They bear living young. If their dispersal were effected to any extent by birds, one would expect to find sporadic colonies, at least in a few cases, instead of the logical and definite distribution which is actually found.[2] Inter-island drift upon trees, here as throughout Polynesia, is excluded by the absence of large streams to transport such materials to the shore. The present currents and trades would not drift from island to island. Finally, the conditions on the shore zone are such that a forest snail thrown upon the strand could not exist.

The Pleistocene deposits of land shells, though only now beginning to be explored, show that even then the island fannas were more closely related than at present. Numerous relatively large species were common to two or more islands, while at the present day there are none. All the facts are explicable on the theory that the islands have been successively isolated by subsidence. Otherwise the distribution of the snails seems enigmatic.

It is obvious that speculation upon the origin and migrations of the Pacific faunas, such as I have been allowing myself to indulge in, will have a vastly broader basis when we have adequate knowledge of the faunas of all the principal high islands. Hypotheses which now seem reasonable may be confirmed, or they may be definitely excluded from further consideration. All the high islands of the Caroline, Austral and Hervey groups should be investigated. In Samoa and the Marquesas we certainly know only a fragment of the fauna. Outlying islands, such as Rapa, specially need examination. On the American side, Juan Fernandez and Mas-a-fuera call for investigation.

At present it may be said that we have a good, though by no means complete knowledge of the snails of the Hawaiian Islands, which resident naturalists are zealously working on. In the Society Islands, through the work of Andrew Garrett, we have a fair knowledge. In Fiji, Tonga and scattered islands of other groups, it is likely that most of the large forms are known, the small and minute scarcely at all. Work on land snails has been sporadic, in large part done by chance visits of naturalists whose chief interest was in other subjects. Everywhere material must be preserved for anatomical study, as the affinities of many land snails can be worked out only from the soft parts, and at present we are guessing at the taxonomy in some of the families. Outside of Hawaii but little material has been collected for anatomical study.

A word as to the 'low islands.' A group of small or minute land snails has been adapted to atolls and the shore zone of high islands.

Many of the species have a wide distribution on different islands or different groups. To be found in any coconut grove, hiding under leaves or wood to which they stick fast in dry weather, they might easily be carried about by human agency. Some groups of small operculate snails, such as Truncatella, live only near the sea, where the salt breeze blows. These seem particularly adapted to distribution by flotsam, yet even they fail on the more outlying islands, some of which have rich faunas of forest snails. The atoll faunas are poor. Thus, Funafuti has 7 species, Palmyra 1; none being peculiar to these islands. Contrast Hawaii, with over 800 species on the 8 islands, or the far less explored Society group with over 100.

Except in Hawaii, such investigation as is needed in the Pacific is beyond the reach of the individual naturalist. It must be done soon, or much of the data will surely be lost forever. The introduction of continental plants and insects, particularly the all-devouring ants, of goats and cattle, of snails and slugs from Europe, Java, Japan and China, has already made serious inroads upon the native faunas and floras. Every year sees the passing of some island species, perhaps a link in the chain of evidence which cannot be replaced. On some small islands, such as Kahoolawe (south of Maui, area 69 square miles), the destruction has already been completed. The native fauna and flora, with whatever they had to tell us, has disappeared in the last 50 years or less, except as we can recover fragments from the aeolian deposits in ravines. Over many miles the very humus has blown away, and desolation like a lunar landscape meets the eye. Such a fate awaits many a Pacific island, we cannot doubt. A comprehensive scientific exploration of these unique faunas must be made while yet it can be done, before they are mutilated beyond restoration.

[1] The snails of the group Orthurethra best known to the general zoological reader are the Achatinellidae, which formed the basis of Gulick's contributions to evolution doctrinex, brought together in his *Evolution, Racial and Habitudinal*, published by the Carnegie Institution of Washington, 1905. Also the Partulidae, which many travelers in Polynesia and Micronesia have noticed.

[2] The data may be found in the writer's *Manual of Conchology*, vols. 21–23.

SOME PROBLEMS OF THE PACIFIC FLORAS

By Douglas H. Campbell

DEPARTMENT OF BOTANY, LELAND STANFORD UNIVERSITY

Read before the Academy, April 17, 1916. Received, July 6, 1916

No botanist, who is at all acquainted with the rich and varied floras of the different regions of the great Pacific area, can fail to be impressed with the numerous important problems that present themselves—many of which have as yet received but scant attention. Perhaps most important of all are those dealing with the origin and distribution of these rich floras.

First, and perhaps foremost, is the collecting of data for a study of the relationships existing between the floras of the great Continental areas of the Southern Hemisphere, viz.: Australasia, South America, and South Africa. These areas, so completely separate at present, nevertheless show such similarities as to suggest a common origin for many of their elements. Such a study involves a consideration of the existing floras, which are pretty well known, but also an investigation of the fossil floras, in regard to which we are by no means so well informed as might be wished. Perhaps the fossils collected in the recent Antarctic Expeditions may help us, and, of course, the investigations of the geologists in regard to possible former land connections must be taken into account.

Second, a study of the floras of the innumerable islands of the Pacific should afford much material for many important generalizations. While our knowledge of the special floras—e.g., of the Galapagos Islands and Hawaii, is reasonably complete, there remains room for work concerning the origin of these floras and their later evolution.

The Hawaiian Islands, especially, offer what is probably the most favorable field in the world for the study of the evolution of plant forms. Hillebrand, in his excellent *Flora of the Hawaiian Islands* gives in the introduction (unfortunately left uncompleted by his untimely death), some most valuable suggestions of the possibilities afforded by a study of the flora of Hawaii. It is possible that further work has been done in regard to this matter since Hillebrand's, but if so, I am not acquainted with it. It is generally believed that these islands have always been thus isolated, and while this is not universally admitted, at any rate any former land connections must have been of a very remote period.

A most striking feature of Hawaii is the different ages of the islands which have been formed in succession, beginning with Kauai at the

northern end of the group. The next and largest island, Hawaii, is still in process of formation.

Hillebrand noted the extremely interesting fact that Kauai, although much smaller than the southern islands, Maui and Hawaii, and with much less range of elevation, is botanically much richer than either of those islands, and contains a much larger proportion of endemic species. It would seem then, that a study of these endemic plants and their distribution in the islands ought to throw much light upon the processes of species formation.

Some of the factors concerned in this remarkable degree of endemism are evident—notably the extreme isolation of the islands; but the agencies that have acted in the islands themselves are not all so clear. The very large percentage of endemic species which, according to Hillebrand, comprises about 80% of the higher plants, suggested to him the possibility of the islands being a centre of special creative energy.

It might be asked perhaps, whether, in newly formed areas, especially those of volcanic origin, there is some factor present which tends to produce variation in plants introduced to them. This is a topic which, as far as I know, has never received attention. It is notorious that volcanic soils are extremely fertile, and some of the richest floras of the world are found in volcanic regions. May it not be possible (this suggestion is offered with some diffidence), that new elements in recent volcanic soils may have an effect, stimulating or otherwise, upon newly introduced plants, causing changes which, fixed by time, result in new species. This is a topic which the numerous volcanic islands of the Pacific should help to elucidate, and one which it seems would be quite worth while investigating.

This question was suggested by observations which I made a few years ago while collecting liverworts in the Malayan region. In Java, which is entirely volcanic in its formation, the liverworts, as well as other types of vegetation reach an astonishing development, both in number and variety, and a comparison of other regions where these plants abound, shows that most of them are volcanic—e.g., Hawaii, New Zealand, the Philippines, Sumatra, and Japan. On the other hand, the granitic formations of the Malay Peninsula and North Borneo, climatically quite similar to Japan and Sumatra, are noticeably deficient in liverworts.

Third, the agencies by which plants reach such remote regions as the Hawaiian Islands, for example, have been repeatedly studied, but very much can still be done, and apparently the amount of accurate experimental work in this direction is not large. Hillebrand thinks a most

important agency is that of certain birds. He mentions specially, a species of plover, and the tropic bird. As is well known, Darwin brings out this point, having examined the mud adherent to the feet of certain migratory birds, in which be found seeds of many kinds. A complete investigation of this topic would probably be worth while. The rôle of air and water currents as agencies in plant distribution, still affords a large field for investigation.

In connection with the study of the island floras, especially those of the volcanic islands, it would be very desirable to have careful observations made on the reëstablishment of the vegetation on areas covered by recent volcanic ash or lava. Such observations, for example, as those made upon Krakatoa after the great eruption of 1883. It would probably not be possible to find so complete a sterilization of a whole island as was then the case, but if by chance any similar occasion should arise, the opportunity should certainly not be lost.

The study of the reëstablishment of the vegetation on such a sterilized area would be of great scientific value; and probably results quite worth while might be had from an investigation of the development of vegetation on less extensive new lava flows, or areas covered with fresh volcanic ash. By such studies it might be possible to determine what effects, if any, are produced upon plants growing in lava or ash, of different chemical or physical properties.

The Pacific Coast of America, both north and south, offers many interesting problems, dealing with various phases of plant origin and distribution, and probably California would prove the richest field for such studies.

Aside from the question of variation in the native plants, and the reasons for the high percentage of endemism involving the question of mutation, etc., there are various other points to be considered. If California, like Hawaii, is to be looked upon as a centre of 'formative activity,' the reasons for this need to be carefully and thoroughly investigated.

California has received additions to its flora both from the north and from the south; and there is much to be learned as to the means by which these immigrants reached California, and just where was the home whence they came. Thus, of the northern elements of the flora there is a small infusion of Asiatic and Eurasian types, such as the Tan-bark Oak, the Giant Equisetum, species of Fritillaria, the Sitka Spruce, Madroño (Arbutus), etc. Probably a thorough study of the fossil plants of the State will throw light on some of these questions, and a beginning has already been made in this direction.

Finally, and this applies to all the regions involved, there is to be taken into account the many species, weeds and cultivated plants, which have been introduced through the agency of man. A study of these immigrants—the origin of most of which can be fairly well ascertained, especially when they have been long established in their new habitat—ought to be one of the best means of determining the effects, if any, of a changed environment. The vexed question of the inheritance of such acquired changes can thus be studied on a large scale and under normal conditions. It is surprising that so little attention has been paid to this great field by the many students of mutation and genetics who set the fashions in biology just now.

These are but a few of the many problems which might be undertaken by the botanist in this vast region which it is proposed to explore. It is perhaps presumptuous for one whose work has been mainly in other directions, to suggest what are the most important lines of work in fields in which he is very much of an amateur.

NERVOUS TRANSMISSION IN SEA-ANEMONES

By G. H. Parker

ZOÖLOGICAL LABORATORY OF THE MUSEUM OF COMPARATIVE ZOÖLOGY AT HARVARD COLLEGE

Received by the Academy, June 1, 1916

The retraction of a sea-anemone like Metridium can be accomplished by stimulating mechanically almost any point on its exterior. Hence there must be nervous connections between all points on the surface open to stimulation and the longitudinal muscles of the mesenteries by which retraction is brought about. To ascertain how extensive these connections are, the following experiments were tried. The column wall of a Metridium was completely girdled by a cut deep enough to penetrate the wall but not sever the mesenteries. Stimulation of either the oral or pedal portion of the partly divided animal resulted in general retraction. Complete removal of the oral half left the pedal half capable of retraction. Tongues of column wall cut in any direction and as long as five centimeters transmit when stimulated at their free ends to the longitudinal muscles thus causing retraction. If a Metridium is cut through vertically except for the pedal disc or a piece of the column or the oral disc, the stimulation of one piece will cause the whole animal to retract. If an oblong outline is cut on the column wall so that the encircled area remains attached to the rest of the animal only by the mesenteries, stimulation of this area is followed

by the retraction of the animal as a whole. It thus appears that the connections between the surface of the sea-anemone and the deep seated muscles concerned with retraction are so numerous and devious that a nervous network is the only basis of explanation. That this nervous network is not equally developed in all parts of the animal's body is seen from the fact that when a sea-anemone is cut vertically in two except for the lips, it is very difficult to get a retraction in one half of the body when the stimulus is applied to the other half. The lips are poor means of transmission compared with other parts of the body. Notwithstanding the generally diffuse condition of the transmission system in Metridium, there is evidence also for a certain degree of specialization in the parts concerned. Stimulation of the tentacles by mussel juice calls forth a gaping of the oesophagus (contraction of the transverse mesenteric muscles) and by weak hydrochloric acid a retraction of the oral disc (contraction of the longitudinal mesenteric muscles). These two forms of response afford good ground not only for the assumption of independent receptors but of relatively independent transmission tracts, a first step in the kind of differentiation so characteristic of the nervous organization in the higher animals.

The extended paper will be published in the *Journal of Experimental Zoölogy*.

THE RESPONSES OF THE TENTACLES OF SEA-ANEMONES

By G. H. Parker

ZOÖLOGICAL LABORATORY OF THE MUSEUM OF COMPARATIVE ZOÖLOGY AT HARVARD COLLEGE

Received by the Academy, June 1, 1916

As long ago as 1879 von Heider announced that tentacles severed from a sea-anemone were capable of much the same range of activities that these organs exhibit when normally attached to the animal. This statement has been variously accepted or questioned by subsequent workers. Favorable material for testing its validity was found in the Bermudian sea-anemone Condylactis. The tentacles of this form may measure as much as 15 cm. in length and may have a basal diameter of 1.5 cm. Severed tentacles from Condylactis contract and remain so for some time. They can be brought to a state of least disturbance by suspending them on a metal hook in seawater. Under such circumstances they can be inflated by gently running seawater into them till they attain about two-thirds their ordinary length. In this condition they are under a pressure of not over a few millimeters of water. If this

internal pressure is much increased, they will contract vigorously and discharge more or less of their contents. The slightly contracted state of the excised tentacle is therefore not due to lack of pressure. It is also not due to the absence of inhibitory influences from the rest of the animal, for, if an attached tentacle is partly cut into at its base, it exhibits the same partial contraction that the excised tentacle does. It is highly probable that the incomplete expansion of severed tentacles is due to the operative complications which are involved in cutting it and which induces a heightened tonicity in its musculature.

When an excised suspended tentacle is stimulated mechanically or by the application of weak acetic acid, mussel juice, or a weak solution of quinine, appropriate responses are called forth that differ from those of the attached tentacles only in that they are feebler and less precise. As this modified form of response is also noticeable in attached tentacles which by previous stimulation have been partly contracted before the particular stimulus was applied, it is believed that the feebleness and lack of precision in the reactions of the excised tentacles is due to their partly contracted condition and not, for instance, to the loss of central influences. Since the excised tentacles exhibit a range of responses like those of the normal ones except in so far as operative conditions modify them, it seems clear that the tentacles of sea-anemones must contain within themselves the neuromuscular mechanism essential to their activities as originally suggested by von Heider.

When various stimuli are applied to the ectoderm of an excised tentacle, they are followed quickly by a muscular response. When they are applied to the entoderm of a tentacle by being injected into its interior, they are also followed by the same form of response but only very slowly. This slowness in reaction is believed to be due to the non-receptive character of the entodermal surface and to the necessity for the stimulating material to the wall of the tentacle before it can reach the receptive ectoderm on the outside. Such a transfusion has been demonstrated in the case of weak acetic acid.

The tentacles of Condylactis like those of other sea-anemones, exhibit polarity. Their cilia beat always from the base of the tentacle towards its tip. If the tentacle is stimulated mechanically at a particular spot, the resultant muscular activity spreads from the stimulated spot almost exclusively toward the base of the tentacle. The ciliary polarity is unaffected by anesthetics, but that of the muscles largely disappears under such treatment. The muscular polarity seems, therefore, to depend upon a nervous condition and is probably due to the fact that the majority of nerve-fibers from the ectodermal sense-cells extend

toward the base of the tentacle, not toward its tip, and thus deliver impulses proximally rather than distally along the tentacle.

From these various observations, it is concluded that the tentacles of sea-anemones, in contradistinction to such appendages as those of the arthropods and of the vertebrates, must be regarded as containing within themselves a complete neuromuscular mechanism by which their responses can be carried out quite independently of the rest of the animal. These organs thus possess great autonomy and act in harmony and unison more because of simultaneous stimulation than of subordination to a common nervous centre.

The full paper will be published in the *Journal of Experimental Zoölogy*.